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INTRODUCTORY LECTURE

TO

A COURSE ON PATHOLOGICAL ANATOMY,

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BY

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GENTLEMEN,—Among the numerous subjects which might be chosen for the theme of an introductory lecture upon Pathological Anatomy, I have ventured to select for brief consideration to-day the influence which the pursuit of that science has exercised upon the advancement of medicine generally. The preliminary observations which it will be necessary for me to make upon the nature and scope of Pathological Anatomy itself, shall be as concise as possible, as the time which I have at my command is scarcely sufficient to allow of more than allusion to many parts of the main subject.

The animal body consists of a combination of solids and liquids, which are constantly undergoing the changes characteristic of life. So long as these changes occur within the limits appropriated to health, their history belongs to the province of physiology; and the study of the structure and composition of the instruments engaged in or affected by those changes, constitutes the subject of that division of anatomy known as *general* or *textural*. But these limits are repeatedly transgressed in consequence of various influences, intrinsic and extrinsic, acting upon the component parts of the frame, and bringing into existence certain conditions incompatible with that free and perfect exercise of function characteristic of health. These conditions supply the materials of the science called pathology. It is the business of pathology to trace their origin, progress, and termination; to determine the causes producing, the phenomena attending, and the effects following them.

In pursuing the investigation of these conditions productive of disordered function, the inquirer discovers that with the rarest exceptions the tissues and organs engaged in performing that function, do not present the characters which he has been accustomed to recognise in health. The functional change is almost invariably connected with appreciable deviation from the natural state of the textures. And as every portion of the economy may be the seat of this deviation, it follows that a system of structural conditions different from those prevailing in health exists. These conditions constitute one of the departments of the science—*morbid* or *pathological anatomy*; and, as you perceive, this department corresponds to the *textural anatomy* of health.

There is a second species of anatomy of the healthy body, known under the title of *descriptive*, with its allied subject *regional*, *topographical*, or *surgical anatomy*; branches of that study having for their object the determination of the size, capacity, and number, the situation, shape, and attitude of the different organs and parts of the body, and their relations to each other in respect of continuity and contiguity. These various properties or qualities are liable to alteration, either as a result of internal disease, or of the action of external agencies; and such alterations form the subject of another department of morbid anatomy.

Lastly, there is a division of anatomy of the healthy body, which has, within a few years comparatively, exercised the genius of a considerable number of inquirers, and which has for its immediate aim the establishment of the phenomena attending the evolution of the tissues and organs—I allude to *developmental* anatomy. The appearances which the pursuit of this species of anatomy teaches us to be characteristic of natural development, are not observed in all individuals; the conditions of growth are liable to swerve from the perfect type, and when thus modified become the subject of a third department of morbid anatomy. Here are to be ranged the various forms of congenital malformation.

It may, however, be not out of place here to remark, that the generation of congenital

deformity is not so invariably and uniformly dependent upon departure from the natural conditions of development, considered *per se*, as it was some few years past the habit to believe. Unbiassed research has of late shown that certain defects of organisation, which had been invariably referred to suspension, or some other anomaly of the developmental process, as their primary cause, originate fundamentally in textural disease, occurring during intra-uterine life, and may or may not acknowledge the secondary and subsidiary influence of an error in that developmental process.

To resume then: the science of pathological anatomy forms the counterpart in the abnormal state of three divisions of anatomy in the natural condition of the body, *descriptive*, *textural*, and *developmental*. In a word, it embraces as its proper subject every material condition, whether congenital or produced after birth, and whether affecting composition, texture, or configuration, which constitutes a departure from the perfect state of the animal structure.

The subjects upon which morbid anatomy works being thus ascertained, it becomes our business to inquire in what manner (or rather to what extent) it should work upon them. Opinions vary upon this point; there are, in fact, scarcely two writers who have touched upon the topic, that take precisely the same view of the legitimate scope of this division of medical science. While some would so unwarrantably contract its sphere as to exclude from it the morbid states of the liquids, others running into the opposite and (as it appears to me) equally unjustifiable extreme, would include within it what has been termed the pathogeny of disease; that is, the doctrine of the origin and mode of production of maladies. Not only would the reference of that doctrine to pathological anatomy be an encroachment upon the ground of general pathology, but the morbid anatomist, by committing the encroachment, would deprive himself of his distinguishing attribute as a cultivator of medical science. He would cease then to deal exclusively with material phenomena, phenomena which come within cognisance of the senses. He would, instead of examining demonstrable facts, be forced to engage deeply in hypothesis; instead of scrutinising the physical and chemical properties of parts with almost the sure correctness of the natural philosopher, he would find himself speculating with the uncertainty of the theoretical nosologist. Now it can scarcely be questioned, under the existing tendencies of scientific research, that it is a matter of no trifling importance to banish from even one of the elements of medical science all forms of speculation—to ensure to ourselves one, at least, in which observation may reign undividedly. I would, therefore, limit, as follows, the field of pure morbid anatomy:—

1. To the determination of the "anatomical character," or special lesion, of all functional departures from the natural state, whether that character exist in the liquids or solids.

2. To the minute scrutiny, both with the unassisted senses and with the aid of microscopical and chemical analysis, of such lesions.

3. To the establishment of the phenomena of their anatomical course,—their origin, progress, and termination.

4. To the examination of the anatomical effects of morbid states of one part upon others, whether these be contiguous to or more or less remote from the former.

5. To the discovery of the alterations of structure of the solids and of composition of the liquids, which originate in the system in consequence of the pre-existence of others.

In a word, I would fix the office of morbid anatomy to tracing the *material* (that is, the physical and chemical) history of disease in all its bearings. This will be rendered perfectly clear by a single example, and as a disease which is well understood, I select phthisis for the purpose of illustration. Our first point, then, is to determine the anatomical character or special lesion of the disease; this is done by the discovery of a peculiar matter called tubercle in the lungs, a matter which is produced in that organ in no other malady. Having ascertained this, our next business is to examine the physical and chemical properties of this matter, and determine whether it be possessed of structure or not, when in its perfect and fully-formed state. We then inquire into its mode of origin in respect of its own primitive condition and the seat of its deposition, trace it in the progress of development, and ascertain to what natural issue it tends. But this matter (tubercle) is not formed in the tissues without the occurrence of certain changes in these tissues—whether immediately in contact with it (the pulmonary substance) or more or less distant from it (the bronchi and pleura)—such changes form the next subject for investigation. Finally, we have to determine, by repeated examinations, whether there are any alterations of structure in remote organs either peculiar to, or forming a necessary element in, the evolution of phthisis. Such alterations we find do exist in the larynx, the trachea, and epiglottis, in the small and large intestines, &c.

With all this to do, in respect of every disease to which the human organism is prone, with phenomena of such grave interest to examine, and of such importance for the conception of the nature (and hence for the successful management) of disease to establish, the pathological anatomist might assuredly rest content. Having (as in the instance I have adduced) traced the material phenomena from the deposition of some particles of adventitious matter in the lungs to

the destruction of tissues in remote parts of the frame, be might, without the consciousness of having made a serious sacrifice, concede to others the task not only of inquiring into the primary cause and original essence of the malady, but of establishing the connection of its anatomical and other phenomena.

And so, in truth, pure and unmixed pathological anatomy may be pursued as a distinct and isolated branch of medical science, and one of unbounded interest for the philosophical investigator. But it is not in this respect that it acquires its chief importance in the eyes of him who is engaged in the treatment of disease—of the practical physician or surgeon; it is as an applied science that its importance shines most conspicuously. Like other branches of medical study, it derives much of its value from its capability of being applied to the elucidation of the rest, from the light it throws upon the phenomena which more immediately belong to *them*.

And with the view of illustrating this important truth, I proceed to consider pathological anatomy in its relations to some of the divisions of medical science; and, first, in respect of the influence it has exercised on the advancement of pathology, or the theory of disease generally.

Two points essential to the establishment of that theory are the *seat* and *nature* of disease; and in order to understand the amount of elucidation they have received from the culture of morbid anatomy, it will be necessary, first, to glance at the information on these heads derived from other sources: these sources are only, so far as I can see, to be found in the principles of etiology and symptomatology.

What, then, will the etiology of disease teach us as to its *seat?* If we except from consideration affections originating in the direct application of a mechanical or chemical agent, no enlightenment as to the locality of disease could accrue from the most diligent, *à priori*, study of its causes. What acumen could have divined that the poison of marsh effluvium would act specially on the spleen? An individual receives a blow on the head, and in consequence of this his liver or lungs become the seat of purulent collections: would the consideration of the cause of these abscesses teach us to foretell their seat? Nay, more, maladies occupying the most various seats originate in the same exciting cause. Of a number of persons exposed to cold and damp when heated, one will suffer from an affection of the pleura, another from disease of the lung, a third of the pericardium, a fourth of the joints; in a fifth the seeds of organic disease of the kidney may be sown: while the majority probably will pass unscathed through the ordeal. Here, then, is sufficient to show that being given the exciting cause of disease, we are still far from being in a condition to pronounce upon its seat.

And from symptoms can we derive more precise instruction?

Symptoms are of two orders: they are general, or they are local.

The general symptoms, those in which the entire frame appears to be more or less implicated, or which can be traced to no particular organ, evidently cannot make us acquainted with the local site of disease. Will the occurrence of rigors, of dryness of skin, of sweating, of rapidity of pulse, of headach, of delirium, of prostration, enable us to lay the finger on the suffering organ? You, Gentlemen, who have observed at the bedside, can readily answer in the negative; neither considered separately, nor in any mode of combination, can they of themselves guide us to the local habitation of disease.

Will the study of *local* symptoms serve us in better cause? I might reject these at once with the simple statement, that maladies of the most destructive character may run their course almost to the end, without giving rise to any manifest local modification of function whatsoever. It is an established fact, for instance, that tuberculisation of the lungs may advance to the formation of cavities without the existence of cough or expectoration. It is equally well ascertained that the stomach may be extensively disorganised by one of the most terrible lesions to which the frame is subject, cancerous infiltration, without having declared itself during life by any notable local disturbance.

I might direct your attention again, among acute affections, to the large class of latent inflammations—to pneumonia cutting off aged subjects in its third stage without having announced its presence by a single pulmonary symptom,—to the lesion of the glands of Peyer, characteristic of one species of continued fever, running its destructive course without local demonstration in subjects who, feeling such trifling ailment as to be with difficulty persuaded to occupy a bed in hospital, perish within a few days with perforation of the intestine and all its terrible consequences.

Let us, however, set these cases aside, and suppose that a local symptom, or group of symptoms, actually exists; we are far from necessarily possessing on this account any sure insight into the point we are considering. Will the existence of dyspnœa, for example (and I choose this symptom because on first thought it might appear distinctly to localise the affection producing it), point out the suffering organ or tissue? Will it teach us whether the source of the difficulty of breathing lies in the lungs, in the pleura, in the bronchi, in the mediastinum, in the spine, in the pericardium, the endocardium, in the great vessels, in the liver, in the spleen, in the stomach, in the ovaries, in the uterus, in the

kidney, &c., for in every one of these parts, and in many others, may reside the efficient cause of dyspnœa? In what forcible relief stands forth the powerlessness of symptomatology when placed in relation with facts such as these! And if it be conceded that there is something in the character of the dyspnœa depending on lesions occupying different seats, which will in many instances allow the experienced clinical observer to exclude some at least of these seats from those probably affected in a given case, how, it may be asked, has he obtained this power? Is it from an *à priori* knowledge that such and such a character must depend upon the implication of any given organ or tissue; for, if so, the importance of pure symptomatology must be at once proclaimed? Indubitably it is not. He has acquired that knowledge by the aid of morbid anatomy,—by comparing the form of dyspnœa observed during life with the seat of disease discovered after death, and by observing a certain association of form and seat a sufficient number of times to warrant a general inference.

Nor are general and local symptoms combined, much less fallacious guides in the attempt to localise disease. For ample demonstration of this we have only to turn to the works of the nosologists of the last century, wherein by disease is understood a certain group of general and local symptoms; and wherein the precise seat of maladies, notwithstanding the eminent sagacity of many of those historians of disease, almost uniformly fails to be determined.

From all this it follows that neither etiology nor symptomatology will of themselves announce the SEAT of disease. We shall presently see to what extent morbid anatomy enables the observer to ascertain it.

Meanwhile, let us pass to the second element in the theory of disease—its nature.

In the existing state of knowledge the phrase "nature of disease" may be understood in two acceptations. A patient dies; we examine his body, and find the anatomical characters, for example, of encephalitis. We conclude that the disease which proved fatal was of the class phlegmasiæ, and in one sense have determined the nature of the malady. Here pathological anatomy is manifestly its discoverer.

But these anatomical characters (that is, the material changes which authorise us in affirming that encephalitis existed during life) do not constitute the final cause of the malady; nor do they explain its essence even when established. Now, a comprehension of these points must be admitted to be absolutely necessary to constitute a perfect doctrine of the nature of disease. Understood in this wide and philosophic sense the nature of maladies is not revealed by pathological anatomy;—all the processes of examination, which advancing science has placed within our reach, fail utterly when interrogated as to the essence of diseases. Let us treat tubercle as we will—analyse it microscopically and chemically—we shall never succeed thereby in displaying the sum of what is meant by scrofulous or phthisical disease. But the motive for surprise here lies not in the fact, that morbid anatomy fails to explain everything belonging to pathological existence, but in the ridiculous pretensions of the so-called organic pathologists, who, in their flush of exultation at the manifest power of morbid anatomy, would define disease "an appreciable change of material constitution," and exclaim, beyond this there is nothing! Is the study of healthy textural anatomy, I would ask, to be pronounced futile,—is it to be discarded, because it teaches us nothing of the cause of the functional acts which the textures perform? Shall we cease to recognise physiological anatomy as a science, because the most minute analysis of the elements of the liver leaves us utterly in the dark as to the cause of their secreting bile? No! Nor, on the other hand, shall we omit to take accurate cognisance of the organic changes arising in those tissues, because their nature constantly fails to announce, *à priori*, the precise functional modification which must ensue. There is a pathological life as there is a physiological life; and if the essence of *this* be still a mystery and a wonder, why should we expect to lift the veil which hides the nature of *that* from our view?

Considered, however, in the humbler and more practical sense, we learn from pathological anatomy, in respect of the nature of maladies, that certain kinds of lesion are connected with certain functional derangements. We ascertain the middle point of what may be called the triune quantity disease; that is, proceeding in the order of development,—first, constitutional aptitude or predisposition; secondly, modification of structural condition; thirdly, local and general disturbance of function. We learn the mode of anatomical change which gives rise to various orders of physiological change; and this is really the ultimate fact of which there appears any existing probability of our comprehending. To penetrate into the mystery of predisposition is, at present, beyond the mark of our legitimate expectations.

Can we attain to this point, or can we acquire any equally valuable instruction from any other order of facts within our reach? Will etiology or symptomatology lead us thus far in the search after the nature of disease? They desert us at the very threshold of the investigation. If they are valueless as means of discovering the precise locality of disease, they are even more so in assisting us to pronounce upon its nature. To recur for an illustration to a symptom already employed, dyspnœa, do we, from the existence of this functional change, considered *per se*,

learn whether (to confine ourselves to the lung only) the patient suffering from it labour under bronchitis, simple or plastic; pneumonia, pleurisy, hydrothorax, emphysema, tubercle, cancer? Pass to the other organs, the morbid states of which are capable of giving rise to dyspnœa, and an equal or greater number of lesions of each are found, which may be possibly attended with that symptom. Nor will any combination of symptoms enable us to determine, independently of pathological anatomy, the nature of the disease under which an organ or organs suffer.

But there are a certain number of affections in which the middle point of the triune existence, disease, is said to be wanting,—in which no anatomical change can be found; and the implied insinuation against the perfection of morbid anatomy as the basis of practical pathology, cannot at this moment be altogether rebutted; there *are* such diseases. I say emphatically *are*, for it is a question whether they will always continue to exist. It seems scarcely comprehensible, in truth, that the disturbance of a function can persist without implication of the instrument performing it. Which is the more likely;—on the one hand, that continued disturbance of function without organic change is a possible occurrence; or, on the other, that organic change really exists in all cases of disease, and only appears to be wanting in some of them, because we are unable to detect it? There can be little hesitation, it appears to me, in adopting the latter as the more probable case. But it may be said that this is an *à priori* argument. I appeal then to experience for its justification. I rest my conviction that in all probability the existence of pure functional or dynamic diseases is destined to be ultimately proved unreal, upon the fact that the number of such diseases has considerably decreased, and continues to decrease, since the study of pathological anatomy has been actively pursued under the guidance of modern science. An example or two may be worth adducing. How greatly has the catalogue of pure functional asthmas been reduced by the discovery of pulmonary emphysema. The discharge of alkaline urine was once believed to constitute an essential disease, and the phosphatic diathesis is even still talked of in this light. But anatomy has shown that this modification in the constitution of the urine depends upon a certain mode of inflammation of the kidney. Numerous forms of dyspepsia, formerly believed to be merely functional, are now known to depend upon structural change in the stomach; and that very common malady of children termed "infantile remittent," and which was long supposed to be a pure idiopathic fever, is now known to consist fundamentally in a sub-inflammation of the mucous membrane of the stomach and intestines. Similar examples might be adduced from the pathology of many other organs.

In the case of these diseases ignorance was the cause of the belief in their purely dynamic character; that ignorance has passed away, and with it the notion of their simple dynamism. The class of diseases termed neuroses may cease to appear merely functional, when we have been some time longer acquainted with the healthy structure of the nerves.

Connected with these diseases are others, which, though accompanied by evident organic change, are alleged to have no *peculiar* anatomical character. The malady called continued fever is one which may be held up to the morbid anatomist, as unattended in all climes and under all circumstances by any one change in the dead, which proves it to have existed in the living. This is *probably* true of the solids under all views of the subject—*certainly* true with respect to the more obvious and palpable alterations of structure. But is there no change in the liquids characteristic of the disease? A multitude of points in the history of the affection strongly indicate the existence of *primary* lesion of the blood. This *primary* lesion of the blood has not been demonstrated, it is true; but here is no evidence of its non-existence, so imperfect is, even still, our acquaintance with the natural constitution of that fluid. A sufficient proof of this, in the direction of microscopic inquiry, appears in the fact, that corpuscules, which are pointed out by one observer as depending on impregnation with pus, are by others regarded as forming natural elements of the fluid. In the chemical history of the blood we are much more deficient. Surely it is not too much to assume that with the cessation of this comparative ignorance must arise new capability of estimating the nature of aberrations from the healthy state.

But, Gentlemen, if some of the facts, which have now been alluded to, show that pathological anatomy is not the alpha and the omega of the science of medicine, considered in its higher and philosophic bearings, they in no wise diminish its all importance in a practical point of view; and I trust my free admission of its existing imperfection in the one respect will absolve me from the imputation of inclining to exaggerate its utility in the other.

Without pathological anatomy, without a high degree of perfection of this, the diagnosis of internal disease must be at one end. This is in the present state of knowledge a manifest truism; yet, as it is one which is too often lost sight of, a few illustrations of the fact may not be misplaced. What should we have known at the bedside of the diseases of the brain, had we not repeated multitudes of times our post-mortem examinations of that viscus? Is there any quality

of genius existing which, guided *only* by symptoms, by signs, and by causes, could have enabled its possessor to say *that* man labours under encephalitis—*this* patient has had hæmorrhage into the substance of the right or left hemisphere—*here* are the functional effects of softening of the organ. Turn again to diseases of the heart and lungs. See the admirable precision with which the clinical observer declares the special tissue which is implicated in a given case,—see how not only the tissue engaged is fixed on, but the division of that tissue, the particular valve diseased, for example, and the nature of its disease, announced with almost unfailing certitude. Turn your thoughts back, then, a few years to the period when all affections of the heart—I might better, perhaps, say of the precordial region—were confounded under the vague titles of aneurism and ossification, when the diagnosis of pericarditis was deemed an impossibility, and the existence of such a malady as endocarditis had not been dreamed of. Having done this, let us inquire into the cause of so rapid and so great a progress, and we shall find in ultimate analysis that it all owes its being to the sedulous cultivation of morbid anatomy.

And in respect of the diagnosis of disease, the power of morbid anatomy is of even a higher order than the greatest possible facility in discovering the anatomical condition of any given organ from *direct* clinical investigation would indicate. It teaches us to predicate in the case of the living subject the influence which certain diseases exercise upon distant organs. Hence it enables us in some instances to affirm, from the existence of anatomical lesions in one organ, that another or others must be the seat of organic change, although we have made no *direct* investigation of the condition of that other or those others. As this capability on the part of the pathological anatomist has arisen out of the improved system of examining the dead of late years prevalent, it will not, I think, be without interest to describe briefly the methods habitually pursued at different eras in performing that examination. In this point of view the study of morbid anatomy is referrible to three distinct periods.

1st. The earliest period, during which that science was cultivated, may be characterised as its marvellous epoch. The actual ignorance which then prevailed as to the nature of disease, coupled with the superstition and credulity of the times, led the early observers to seek for wonders in all directions. "De abditis nonnullis ac *mirandis* morborum et sanationum causis," was the title of what may be called the first work on morbid anatomy, that of Benivieni. The changes in the anatomical constitution of the body attending common diseases were thought unworthy of scrutiny,—affections presenting some extraordinary features alone deemed fitting subjects of inquiry. The reality of the things described—the possibility of their occurrence even—too often became matters of secondary importance; the main point was that the marvellous should predominate. Hence the fabulous narrations to be found among the very early writers; hence the stories, for example, of "a man born of a goat," of "a woman who brought forth an egg," and an abundant array of similar marvels.

And this love of the extraordinary, though it ceased at an early period to betray men into such gross extravagancies as these, nevertheless continued to give (long after the general character of pathological investigation had changed) its tone to literature. The fabulous was, it is true, discarded; but the strange and uncommon continued to be regarded with especial interest, and morbid anatomy to be pursued with a view to the accumulation of facts curious from their rarity, rather than instructive from the light they threw on the phenomena of disease generally.

2nd. With the advance of general knowledge and the decrease of veneration for the marvellous, the indulgence in such absurdities as I have referred to was given up. An important change took place—the *seat* and *cause* of the disease under which patients had succumbed, became the objects of inquiry; hence the organ, which had by the symptoms present during life been proclaimed at fault, was regularly examined, with a degree of minuteness proportional to the acquirements of the period. Our own immortal Harvey, perhaps, led the way in this reform. He opened a great number of bodies with a view to the detection of the locality and the nature of diseases; but as he did not live to embody his researches in a work, the honour he might have reaped has passed away to another. Our countryman Willis, Wepfer, Fernel, Sylvius, and Baillou, made important additions to knowledge upon special diseases. Bonetus taking a wider field produced his celebrated *Sepulchretum*; a repertory of the then presumed facts of the science; disfigured, however, by occasional extravagance, and in a slight degree, only, founded on his own experience. This change in the mode of pursuing pathological investigation may then, without injustice, be said to have been established by Morgagni. In the immense accumulation of facts collected by that indefatigable observer, we find the same practical aim invariably conspicuous,—that of explaining the circumstances of the disease by the seat and nature of the anatomical lesions discovered in the organ or system, the functions of which had been disordered. But beyond this, investigation was not, unless accidentally, carried. It was deemed a matter of no moment to examine into the condition of organs, which had not been supposed diseased during life; and

hence arose errors of serious character and frequent occurrence. Hence lesions which had no share, or but a remote and contingent one, in the production of the fatal event, were regarded as its whole cause, provided they existed in the organ which had attracted attention during life; while, on the other hand, morbid states of the greatest importance were necessarily passed over, because seated in other parts of the frame. Even far into the present century no other method than this prevailed,—that is, was systematically and habitually followed.

3rd. Like the majority of solid improvements, which have from time to time been effected in the science of medicine, the change to a more enlarged system of examining the organs of the dead was *gradually* effected. Scattered cases in which the condition of the majority of the organs was noted began to appear; the importance of this mode of research ceased to be contested, both in this country and abroad. But it was reserved for M. Louis to demonstrate fully and completely the fundamental importance of submitting to close scrutiny *all* the organs of the dead, in *all* cases indiscriminately, without reference to the supposed seat or nature of the previous disease.

The first effect of this mode of procedure was, that every lesion existing in each case was, so far as the state of knowledge admitted of this, accurately ascertained; the serious fallacy of invariably ascribing death to some or other unnatural condition of the organ supplying the most notable symptoms during life was at once exposed. It was ascertained, for example, (and these instances are mentioned as the first that occur to me,) that patients suffering from pulmonary consumption might in reality owe their death to implication of the intestine; that subjects labouring under croup might die of pneumonia; that individuals long worn by chronic disease, might perish of acute intercurrent inflammation of parts more or less unconnected with the seat of their original malady, &c.

But this was simple. It required no systematic perseverance in the examination of all the organs of an extensive series of subjects to discover the fact. A few fortunate chances must have sufficed to make it matter of demonstration; and hence it was by no means unknown as an occasional occurrence, long before the method of pursuing pathological investigation, to which I now advert, had been systematically practised.

But by a steady persistence in examining all the organs, it came to be observed that it was not matter of accident, this implication of parts more or less remote from, and functionally more or less independent of, the seat of primary disorder. It was found that certain of these lesions occurred only when the primary affection was of a given nature, and occupied a given organ. Hence came the inferences that certain secondary changes are peculiar to given maladies; and that anatomical change of determinable parts constitutes a necessary phenomenon in the evolution of disease in others. The doctrine of secondary lesions was thus established.

The light thrown on the natural history of disease by the discovery of the law of secondary lesions, strikingly important as it is, is by no means the most important service it has rendered the physician. The knowledge obtainable from it may be used as a means of diagnosis,—the more valuable because often the sole one. I shall terminate this digression by giving a single example of the fact.

In the course of his pathological investigations M. Louis found that in a very large proportion of cases of acute disease of all kinds attended with febrile action, the mucous membrane of the stomach becomes, at a variable period of that affection, the seat of an anatomical change, proportional in severity and extent to the predisposition of the subject. The importance of this lesion of the stomach is sufficiently manifested by the fact, that in fatal cases it decidedly either hastens, to a greater or less extent, the fatal issue of the case, or, in some instances, actually proves the cause of death. Here, then, is a fact of extreme importance, which never could have been discovered but by the examination of all the organs, without exception. In respect of practical medicine, we learn from it that there is reason to expect, and we should be most watchful for the demonstrations of such change, when febrile action has persisted beyond a certain period. And without the knowledge of the law, we should in numerous cases have no motive even to suspect the existence of such lesion; because concomitant cerebral affection, blunting the perceptions of the patient, many times deprives us of the information derivable in ordinary cases from the occurrence of local pain and functional disturbance.

So much, then, for the influence of morbid anatomy in advancing the diagnosis of disease. Let us next turn to its influence upon the art of prognosis—upon the art of foretelling the course, duration, and issue of disease. It does not require lengthened experience to ascertain how intimately dependent, on the correctness of the physician's verdict in these respects, is, often, the happiness of individuals and the peace of families; and, consequently, how closely his own reputation is linked with the eventual accuracy of his decisions. And upon what are these to be based? Is it upon an abstract estimate of the course, the peculiar character, the severity, the long continuance of any symptom or symptoms, independently of the anatomical lesions attending them? A more faithless guide could scarcely be chosen. Not only will it often afford no correct information, but may actually lead us to believe that the exact contrary of what has really occurred

has taken place. Take the example of a subject labouring under incipient inflammation of the lungs, who has a few hours since undergone copious loss of blood: he expresses himself relieved, he looks the picture of gratitude and confidence; his statement is confirmed by an examination of symptoms, he coughs less, his respiration is less accelerated, he suffers comparatively no pain in the side, his pulse, his skin have less of the febrile character: in a word, the improvement is not an imagination of an anxious patient,—it is real, it exists. What shall prevent the physician, who implicitly trusts to the functional phenomena as his guide in respect of prognosis, from laying the flattering unction to his soul that he has strangled the pneumonia at its birth, and from announcing the feat to the sufferer and his family. But, alas! all three would be doomed, almost certainly, to disappointment; pneumonia is rarely to be thus conjured away. Had the physician examined into the anatomy of the diseased lung, before he sang his *Io triumphe*,—had he marked, by the aid of the physical signs, the boundaries and anatomy of the disease, he would have found (in the great majority of cases) that the real disease had increased instead of diminished,—that where crepitation only was before audible, blowing respiration, bronchophony, and marked dulness on percussion were now present; and that crepitant rhonchus disclosed itself in a portion of the organ, where it had previously been imperceptible. Hence he would conclude that the amelioration depended simply on the abstraction of blood through its influence on the organism generally, and that the ultimate issue of the case (as has repeatedly been proved under the circumstances supposed) was still matter of gravest doubt.

Under circumstances such as these, which, I would ask, is the better promoter of his own interests, which the more useful servant of his fellow-men? Is it he who bases his judgments respecting the course of disease on its mere symptoms, and is thereby drawn into inevitable error, or he who, ascending more closely to the sources of truth, finds the motives of his opinions in that which, as knowledge now exists, forms the ultimate term of the problem disease, namely, morbid anatomy? It is needless to reply. Yet, Gentlemen, by a most palpable contradiction, it is the former who is too often dignified with the title of *practical*. It is the routine symptomatologist who usurps the name which can in common sense belong to the profound master of pathology alone; it is the man who systematically makes light of progress that assumes its highest attribute —the practical—as his own. How grievous it is to see that title awarded with the fullest force of the *lucus a non lucendo* principle—he who deserves it least shall have it most. How dispiriting to find that instead of being a dignity to which the clinical observer may look as the high reward of prolonged and patient toil, it is an empty sound, which, such is its current application, could confer no honour upon him who really merits it.

We have next to consider the influence of morbid anatomy on the treatment of disease: and, first, let me freely acknowledge that there are numerous affections in the treatment of which we derive no assistance from the inspection of the dead. Indubitably we do not owe our knowledge of the power of mercury over syphilis to examination of the lesions of subjects dying with that disease. The efficacy of bark in checking intermittent fever, and diminishing the enlargement of the spleen attending this, could not have been deduced from post-mortem examination of the tissues; or, at least, we do not, in the present state of knowledge, see how it could have been so deduced: yet certain recent discoveries justify us in looking forward to the day when many seeming difficulties of this kind will have ceased to be insurmountable. A few years since when, as now, we cured scabies with sulphur applications, had we been asked for a motive for our treatment founded upon morbid anatomy, we should have been forced to admit our inability to furnish it. Now we can give it; we can point to the insect causing the disease, and say sulphur cures scabies, because it poisons this animal. We can go further; we can tell why (as experience has proved) fumigations should in many cases be the best mode of administering the drug, because in this way it may penetrate more readily to the nidus of the insect; and we can affirm that if an agent be found capable of destroying the life of the acarus more rapidly than sulphur, that agent is better fitted for the treatment of scabies. Precisely the same history is to be narrated respecting porrigo; the production of vegetable matter in this malady explains the effects of numerous applications we have been in the habit of employing, and points to the direction in which improvement of existing modes of treatment may be looked for.

Still there is the class of diseases—the neuroses—upon the treatment of which morbid anatomy throws no light, the anatomical characters of which are unknown. Were they known, it is but just to presume that they would advance our knowledge of their treatment; and at all events it must not be forgotten, that it is precisely of those affections for which no anatomical characters have been found, that the treatment is most pointless and unsatisfactory.

But with the great mass of diseases we shall find a different state of things; we shall find the influence of pathological anatomy no less marked and beneficial than in the other aspects in which we have considered it. To commence with diseases of the chest. Without morbid anatomy, could we have reached

the degree of precision which now prevails in the treatment of thoracic inflammations; could we without it and the physical signs *(which are nothing more than means or instruments of pursuing morbid anatomy on the living)*; could we, I say, have learned that inflammation of one tissue of the lung will almost inevitably require more active antiphlogistic treatment than of another, as we know to be the case with inflammation of its parenchyma and tubes? Could we have anticipated the different amount of benefit to be obtained by the use of tartarised antimony in these two phlegmasiæ? What should we have had to guide us, were we without the aid of morbid anatomy and the observations founded upon it? The local and the general symptoms? These might, no doubt, in many cases furnish fair motive for the regulation of treatment; but how often would they deceive us! It is notorious, for example, that the constitutional reaction is in some cases of catarrh more violent than in others of pneumonia.

But these, and the numerous similar cases which the practice of medicine makes us acquainted with, turn upon delicate distinctions; with or without pathological anatomy the *basis* of treatment is admitted to be known, its activity only is considered to be matter for determination. Morbid anatomy here labours under a disadvantage, and we ought to turn, in order to see its full value, to the period when empiricism held undisputed sway. Then was the time when, for example, the practitioner was taught to cure pleurisy with goat's blood in small doses, "obtained by hanging the animal by the horns, cutting out the testes, and receiving the blood for use." Such a prescription savours of the raving of charlatanism; nevertheless it came from a most orthodox quarter, from Rivière, who was dean of the faculty of Montpellier in 1680, then perhaps the most celebrated medical school of France. Similar nostrums are to be found in numbers in the writings of that period.

But not only has morbid anatomy taught us to put no faith in such absurdities as these, but it is constantly supplying the means of framing a rational system of therapeutics. See the light that has been thrown upon the treatment of dyspepsia by the pursuit of morbid anatomy, by the distinction which this has taught us between cases originating in chronic inflammation, and independently of this process. Has the treatment of dropsy gained nothing from our existing intimacy with its organic causes? Do we derive no advantage, in a therapeutical point of view, from being able (as we are in many cases) to distinguish between serous collection in the peritoneal cavity and in an ovarian cyst? Will not, again, our treatment differ when a fluid collection occupies a single cyst in the ovary, or is seated in a compound or in a multilocular one? The treatment of renal diseases, again, has certainly made marked progress, in consequence of the light of late years thrown on their morbid anatomy.

The practice of surgery abounds with evidences of the influence of morbid anatomy on the treatment of the affections falling within its sphere. What should we have known of the distinctions of various herniæ from each other, and hence of the mode of operating upon them, without its aid? How, otherwise, have learned the practice of irritating the tunica vaginalis for the cure of hydrocele? Is the treatment of dislocations founded upon a perfect acquaintance with the anatomical changes occurring in the displaced parts, or do we pull as chance directs, when we proceed to restore the bone to its natural site? Has the light which even recent research has thrown upon the process of reparation and adhesion, introduced no useful modification into the management of solutions of continuity? Was it not under the auspices of profound morbid anatomical investigation that Dupuytren perfected his operation for artificial anus? Without the most unlimited confidence in the indications derivable from morbid anatomy, could Béclard (as he once did) have dared to cut through the meninges of a subject whom he had trepanned, plunge his bistoury deeply into the substance of the brain, empty the contents of an abscess, and, as the event proved, save his patient from a destruction that must otherwise have been inevitable?

Look again at the hints thus given in obstetric medicine. The study of distorted pelves has taught the precise conditions of size, which will permit the passage of the fully-developed fœtus, and marked out the cases in which premature delivery must be effected, in order to preserve the existence of mother and of child.

These illustrations of influence on the therapeutical art all exemplify the positive good pathological anatomy effects. But there is a negative good accomplished by its aid, which should by no means be forgotten. Through it we have gained our knowledge of that unfortunately too abundant class of organic maladies, for the removal of which art must strive in vain: we have learned to avoid, by abstaining from useless efforts to cure, those dangerous risks which the mere symptomatologist, animated by misguided zeal, must incur, and to limit our endeavours in such hopeless cases to the still important office of the palliation of suffering.

Such, Gentlemen, imperfectly sketched, are some of the views which tend to dignify the study of morbid anatomy, and display its title to be considered the basis of existing scientific and practical medicine. In seeking to enforce its claims to attention, I have felt that strong language is little needed

within the walls of this institution. *Here*, at least, that science has been duly recognised and methodically cultivated. On the one hand, the governing body of this college, ever active in the cause of intellectual progress, were not only in this country the first to establish a chair especially devoted to the subject; but they anticipated even our zealous neighbours the French in the elevation of the study to a distinct professional department.* On the other hand, the chair was filled and the science taught in this college by a pathologist of whom I may affirm (without wishing to detract in the smallest degree from the high and well-earned reputation of many of our countrymen) that he did more than all others, by his admirable "Illustrations," to popularise the study of morbid anatomy among the profession in England. To the enlightened labours of Dr. Carswell, English medicine owes a deep and lasting debt, one which could only be repaid that high-minded observer by the sedulous cultivation, on the part of his brethren, of that branch of medicine on which his toil was spent, and to which his scientific tastes pointed. And it is most gratifying to find that his example has not been lost upon those brought more immediately within its influence. The published debates of the University College Medical Society may be confidently appealed to, as affording the best kind of evidence that the study of pathological anatomy has been pursued with sustained vigour and success by those who acquired their knowledge of its principles within these walls.

It is also a circumstance of no mean import with respect to the future prospects of morbid anatomy in this country, that the public examining bodies appear at length to have understood how important an influence due acquaintance with its facts and principles must exercise on the efficiency of the future practitioner of medicine and surgery. The University of London commences this year to examine candidates upon "pathological anatomy" as a distinct subject. Here is an innovation which cannot fail to promote, in a high degree, the cultivation of this branch of inquiry, and one for which the followers of that practical medicine, founded on science, have strong reason to be grateful.

Before we separate, Gentlemen, let me entreat your indulgence for one moment. Feeling, as I trust I have shown that I do, the high importance of the subject of morbid anatomy, and sensibly alive as I am to the eminent merits of my predecessor in this chair, it is not to be wondered that I should undertake its arduous duties with anxiety and diffidence. But allow me to assure you most sincerely, that whatever my defects may be, they shall not be those of negligence or inattention to your interests. So far as my humble abilities go, they shall be enlisted cordially in your service; and such knowledge as I possess I shall always feel flattered by being called upon to impart in the form of explanation or illustration of the subjects considered in my lectures.

* The early and active devotion of Dr. Hodgkin to the study of morbid anatomy, has ensured for him a place among the most distinguished of its cultivators, in this or any other country.

CASE OF SHORT FUNIS,

WITH SOME GENERAL OBSERVATIONS.

To the Editor of THE LANCET.

SIR,—As the annexed notes of a case, which has recently come under my notice in company with Mr. Blamey, may be deemed of some practical importance, perhaps you would oblige me by giving them a place in your next publication. I remain, Sir, your obedient servant,

JAMES B. THOMPSON, M.D.

London, May 20, 1842.

Mrs. Gelder, of 44, Seymour-street, Euston-square, ætat. 35, of a low stature, was, in Jan. last, delivered of an unusually large male child, her first confinement; she was only four hours ill. The placenta came away in four minutes, and she had in every respect a most favourable accouchement. What induces me to submit this case to your notice, is the circumstance of the shortness of the funis, which when measured was found to be only *seven and a half inches in length*. Now here was a case which one would anticipate to be tedious, more particularly when the age, stature, and other circumstances of the patient are taken into consideration: whereas it was the shortest and quickest I have ever witnessed, in public or private practice. I find, on referring to some former dispensary reports made by myself, that out of three hundred cases of midwifery, the shortest funis was thirteen inches, the longest was forty-eight inches; the most frequent was twenty-one inches, and the next most frequent was twenty-six inches. On referring to a statistical report of my friend Dr. Churchill, referred to in his most practical work, I find that he says, out of five hundred cases, the shortest cord was twelve inches, the longest fifty-four inches; the most frequent length was eighteen inches; the next most frequent was twenty-four inches. Mr. Stone makes mention of some cases in which the cord was not more than six inches in length, and in which the abdomen had been torn from it. Mr. Streeter exhibited to the Westminster Medical Society a cord forty-five inches in length.

In the three hundred cases already alluded to, there were twenty-two cases of first confinement, and the majority were of a duration varying from eight to thirty hours. The membranes were ruptured in from half an hour to two hours previous to the birth of the child. The placenta came away in from eight to thirty minutes. In five cases it was expelled into the vagina as the child was born; and in two cases it was expelled along with the child, and no very serious hæmorrhage ensued, as would be very naturally supposed from such unfrequent occurrence.

I would here draw the attention of the profession to the fact of the similarity in the duration or period of time between the birth of the child and the expulsion of the placenta; for I have been often struck with the remarkable coincidence that in the ratio of the duration of labour, so will invariably the expulsion or retention of the after-birth be regulated. If the one be *quick* or *tedious*, so is the other. This, of course, may not always be found to be so, but I have so frequently noticed it, that I am justified in introducing it here, with a view that others may, at some future period, corroborate or deny its frequency, as far as their own observations may enable them so to do.

CASE OF FÆCAL VOMITING DURING THIRTY-FOUR DAYS.

POLYPUS OF THE INTESTINE, INTUS-SUSCEPTION, AND RUPTURE.

By J. Stewart Allen, Esq., Assistant-Surgeon, St. Marylebone Infirmary.

Anna Regus, ætat. 38, was first visited on the 3rd of February last; she complained of a severe but intermitting pain in the epigastric region, which was increased by pressure. She stated that her health was generally good, but that of late she had been obliged frequently to have recourse to purgative medicines. The pulse was not affected, nor was there any constitutional disturbance. A dose of calomel and opium was prescribed, to be followed by a dose of castor-oil. On the following day the pain had considerably increased; the bowels had not been acted on; she had frequent and violent eructations; and towards evening there was copious vomiting, of a green, fœtid, bilious matter; pulse 70; tongue red and rather dry. Twenty-four leeches were directed to be applied. Calomel and opium every six hours; a common enema to be administered, and repeated until the bowels were acted on.

5. No relief from the leeches; the enema brought nothing away, but they gave great temporary relief; continues to vomit large quantities of a fœtid, yellowish, chalky matter. More leeches were applied, but they also gave no relief.

7. Much worse, the pain recurring in violent paroxysms; the spasmodic action going on in the intestines can be felt by placing the hand on the abdomen; they press in knots against the abdominal parietes; friction gives relief; countenance anxious; pulse 85. Was bled to sixteen ounces.

8. Did not vomit for nearly twelve hours after being bled; is easier, and slept for some time during the night; towards evening there was a return of the violent pain. She was again bled to twenty ounces, which produced syncope.

9. Gums affected by the mercury; pain abated.

10. Apparently sinking; countenance collapsed; extremities cold; pulse scarcely perceptible at the wrist. To have brandy and stimulants: did not vomit for some hours after taking the brandy.

11. Considerably revived. To continue six ounces of brandy daily: to have beef-tea, and an enema to be administered twice daily.

12. Abdomen became covered with large, round vesicles, or rather pustules.

From this time until the 20th there was no material alteration. She continued to vomit daily large quantities of fæcal matter, of the same yellowish, white appearance: the brandy had always the effect of relieving the sickness. That and the beef-tea were the only nutriment, solid or fluid, which she did not immediately eject. On the 20th the enema brought away a large quantity of fæcal matter, but the vomiting continued: no difference could be distinguished between the ejection and the dejection.

From this time until the 28th she continued apparently to make great progress towards recovery; the vomiting, however, continued, although not so frequent, and large quantities of fæcal matter were brought away with each enema.

On the 1st of March she had passed twenty-four hours without having vomited; the bowels had acted three times without the aid of enemas; she was able to sit up in bed; the countenance had lost every trace of anxiety; and she wished for permission to leave the bed.

On the 2nd the vomiting returned; the bowels had acted once of their own accord. The enemas were again directed to be administered, owing to the great relief which they gave.

From this until the 9th the vomiting continued, with little alteration in the other symptoms; the abdomen again became covered with large pustules, nearly the size of a sixpence; and a large patch of integument below, and to the right side of the umbilicus, became discoloured: it is in this region she complains of most pain.

On the morning of the 9th, whilst sitting up in bed, she was suddenly seized with intense pain and shivering; when visited about an hour after, the pulse could not be counted, and was intermittent; abdomen tympanitic; extremities cold; countenance collapsed. Recourse was had to stimulants and sedatives, enemas of warm oil, mustard sinapisms, &c., but she never rallied. Death took place at about two o'clock on the morning of the 10th.

Examination of the Body Fourteen Hours after Death.

On opening the abdomen there was an escape of fœtid air, and a quantity of thin, frothy, fæcal matter was found in the cavity. A small rupture was found in the lower part of the jejunum. The intestine, for about sixteen inches, was in a gangrenous state; the finger could be readily pushed through its coats: immediately below the rupture in the intestine, and occupying its whole calibre, was a solid fleshy substance, about three inches in length; it had grown from the mucous membrane by a very narrow neck; when cut into, it presented a firm, fibrous appearance. The polypus, or tumour, from its weight, had dragged down a considerable portion of intestine, forming intus-susception. The liver was enlarged; but the other abdominal viscera healthy. The gangrenous portion of the intestine was lying in contact with that part of the abdominal parietes where the discoloured patch had been observed during life.

May 24, 1842.

VAGINAL DISCHARGES,

ACCOMPANIED WITH URETHRAL IRRITATION.

To the Editor of THE LANCET.

SIR,—Mr. Lane, in his valuable lectures on syphilis, now in course of publication in THE LANCET, when speaking of vaginal discharges, at page 39 of the current volume, although he expresses himself guardedly, appears to incline to the opinion, that urethral irritation will be present or absent according as the vaginal discharge be of a contagious or a non-contagious nature. As a general rule, this will probably be found to be the case; but if exceptions occur (and doubtless many of your readers have met with cases that would prove such), the value of the symptom, as a diagnostic, will be materially diminished; and I think that the young practitioner, at all events, will do well not to attach too much importance to it, as an erroneous opinion, in a case of vaginal discharge, might, under peculiar circumstances, prove highly detrimental to his interests and professional reputation.

I am induced to make these remarks from having met with cases of vaginal discharge, accompanied with urethral irritation, which, I am fully persuaded, were not the result of contagion; an outline of one of these, which appears to me to possess some interesting and instructive features, although the treatment adopted did not prevent a fatal termination, I purpose laying before your readers. Before relating the case, I wish to make a few observations relative to the extension of inflammatory action in mucous membranes. Mr. L. says in the paragraph above referred to,—"It will also be readily understood that the gonorrhœal matter, when transferred from the male to the female, could not fail of coming in contact with the vagina, and would be soon communicated from thence to the female urethra, as the mucous lining of the two canals is continuous;" from which it is evident the lecturer believes the urethral irritation results from the direct application of infectious matter to the orifice of that canal: consequently the continuity of the mucous membrane is a point of no importance, as the result would be the same, whether the walls of the vagina between the orifice of the canal and the infected spot were lined with tinfoil, or mucous membrane, as from the contiguity of the orifices of the two canals the gonorrhœal fluid, flowing from the vagina, could hardly fail of coming in contact with the orifice of the urethra. The question, however, is this, Does the extension of the disease to the urethra necessarily depend upon the direct application of infectious matter to this part? or, Does the inflammatory action (the result of infection), set up in a part of the vagina with which infectious matter has come in contact, spread along the continuous mucous membrane to the urethra?

If an affirmative answer be returned to the first question, a negative must be given to the second, which would imply that gonorrhœal differs from common inflammation not only in its cause and product (contagious matter), but also in its general characters and the laws which govern it, of which proofs are wanting. The well-known tendency of common inflammation of mucous membranes to diffuse itself over a more or less extended surface, depends, doubtless, upon the general constitution of the individual attacked by it; of this daily examples present themselves in catarrhal affections of the air-passages, the irritation in one being confined to the nostrils, in others affecting the fauces also; whilst in numerous cases it gradually extends down the larynx and trachea, and in some to the ultimate divisions of the bronchial tubes: such, likewise, is the progress of inflammation resulting from specific causes. In some cases the gonorrhœal affection involves only an inch or two of the mucous membrane of the male urethra; in others it extends to the bladder and testes, to which structures we can hardly conceive the infectious matter to be conveyed; whilst in irritable subjects the inflammatory action

set up in the urethra, in consequence of passing a catheter, differs little, if at all, from a gonorrhœal affection, except in its cause and product.

The mucous membrane lining the vagina, urethra, &c. being continuous, we might reasonably suppose that any constitutional cause, acting with sufficient intensity to excite inflammation in this membrane, would operate not only on the vaginal but on the urethral and labial portions also; and in all probability the reason of the general absence of ardor urinæ, in cases of profluvium vaginæ of a non-contagious character, is, that the latter is comparatively rarely the result of acute inflammatory action in the affected part, but usually depends upon a congested state, or upon atonic or chronic inflammation of the membrane: whereas gonorrhœal inflammation almost invariably at its commencement partakes of an acute character. Hence it appears that ardor urinæ may reasonably be expected to be present in cases of acute or subacute inflammation of the mucous membrane of the vagina, whether sporadic or the result of contagion, except where it results from mechanical injury, or the local application of irritants in persons of sound constitution; under which circumstances the affection will probably be circumscribed.

The case which I now propose to relate, occurred in the person of Miss ——, ætat. 20, of delicate complexion and slender frame. About two years before the attack her brother died of phthisis, at which time she was healthy and plump. After this event her spirits were much depressed, and she began to lose flesh. The following winter she suffered from cold and cough, in consequence of which she was advised, as a precautionary measure, to confine herself to the house during the winter and early spring months. She recovered from this attack (which does not seem to have been severe) and continued well until about Christmas, 1838, when, sitting at an open window on a mild day, she caught a severe cold: she had cough, which in the course of about a fortnight ceased spontaneously. A profuse vaginal discharge also commenced, which the mother of my patient said they supposed to be only "a cold," and thought little of it, concluding it would pass off. In this expectation they were, however, disappointed, and at the latter end of March I was requested to visit her. She was now thin, and had the appearance of having been out of health for some time. The tongue was coated towards its root with a thin, dirty-brownish fur; the tip and edges were red, and the anterior portion of the surface tolerably clean; there was slight thirst, and but little appetite; the pulse 80, small, soft, and weak; bowels rather relaxed; motions dark and offensive; menses pretty regular; urine copious and cloudy; no ardor; but the patient was obliged to rise four or five times during the night to relieve the bladder. On expelling the last drops of urine she complained of acute pain of the genital organs, which from its intensity often caused her to cry out; she also suffered from frequent severe attacks of itching and smarting of the external organs, and from an internal smarting, which she could not describe, but which was more severe for some time after changing her position in bed: there was a slight vaginal discharge, which stiffened but did not mark the linen. The irritability of the bladder had existed from the commencement of the malady; and in reply to my inquiries, the patient stated she at first suffered much from smarting on passing her urine. This gradually subsided, and the sharp pain only, above mentioned, remained. She has been getting thin from the commencement of the disease. She complained much of want of rest, she not having slept more than two hours at a time since Christmas, in consequence of the irritable state of the bladder. Although the disease was strongly suspected to be of a serious nature, no examination of the suffering organs took place at this time, the patient and her parent thinking it unnecessary; the case was, therefore, treated as a slight inflammatory affection of the sexual organs. Alterative doses of mercury, combined with Dover's powder, were taken at night, and saline and gentle laxative medicines during the day. Plain but not low diet was ordered, quietude enjoined, and the hip-bath directed to be used every night. No benefit having been derived from these measures after a trial of three weeks, an examination was proposed and assented to.

The external organs were healthy, there being neither redness, excoriation, nor papular eruptions, to account for the severe itching and smarting; the vagina both at its orifice and throughout the whole extent of the canal was highly sensitive, and though the utmost gentleness was used, the examination was severely painful; the os uteri was much swollen, tense and tender; this was stated, the instant it was touched, to be the seat of the internal smarting before mentioned; the cervix was thickly bedewed with the white tenacious mucus, described by Sir C. M. Clarke as pathognomonic of inflammation of this part; no tenderness was felt on the application of firm pressure above the pubes, although the patient occasionally complained of dull pain above or upon the symphisis. The state of the vagina and uterus was sufficient to account for all the symptoms, the contiguity of the bladder and urethra to the inflamed uterus and vagina, would account for the irritability of the former organ; whilst the cloudy appearance of the urine might be fairly attributed to its commixture with the opake mucus, before mentioned, which when washed from the finger rendered the water turbid. Leeches

were now applied to the groins, and repeated six days afterwards; emollient injections were used, and astringents after the second application of leeches; recumbency was enjoined; medicines were continued as before, and the bath alternate nights. In the course of a fortnight, under this plan of treatment, all the more urgent symptoms subsided; and on instituting a second examination, three weeks after the first, the vagina and uterus were found to be in a perfectly healthy state, excepting a slight induration of a portion of the anterior lip of the os uteri: still the slight febrile symptoms persisted, the urine continued cloudy, and the bladder was equally irritable. The patient complained of no pain now, excepting the dull pain about the pubes occasionally: it was now clear that the bladder was diseased, and the appearance in the urine, twice, about this time of a small body, resembling a drop of coagulated black blood mixed with fibrine, completely established the point. Emollient injections were now daily thrown into the bladder; balsamic medicines were had recourse to, with quinine and other tonics, but without benefit. Two or three small films of lymph, about a line and a half or two lines long by a line in width, as if washed from the surface of an ulcer, came away daily with the injected fluid. Astringent injections were tried, but could not be borne; however weak they were used, they caused great pain in the bladder, just above the entrance of the urethra. Not more than two or three ounces of fluid could at any time be retained by the patient; and any attempt to distend the bladder, produced a tearing sensation behind the symphisis. No favourable effect was produced upon the vesical affection by the means used; the patient continued much in the same state from the middle of April until Midsummer, suffering but little except from want of sleep. She was enabled to take walking exercise daily, but could not make use of an invalid's chair, as the vibration caused a painful dragging sensation at the neck of the bladder: she became more and more emaciated; the state of the tongue and pulse did not vary; the alvine excretions were of a tolerably healthy character after the first month of treatment, but the menses did not appear after my attendance commenced. At the latter end of June a somewhat severe access of fever occurred, attended with violent vomiting and diarrhœa, which continued for some days, and reduced the patient's strength very much. She was now seen by an eminent physician, who gave an unfavourable prognosis; he suspected the existence of tubercles, but could not detect their presence in the lungs. The tonic plan of treatment was directed to be continued, but no benefit was derived from the various ordinary tonics, balsams, &c. Hectic flushes became more marked; the strength gradually, but slowly, declined; but no marked change took place until the 1st of August, when, after the bladder had been injected, about a small teaspoonful of dark, unhealthy, offensive pus, with one or two drops of blood intermixed with it, escaped through the canula. From this time the patient kept to her bed; a considerable and increasing quantity of offensive pus was daily passed with the urine; the bladder became so irritable, as not to tolerate even warm water injections, and required to be relieved almost every half hour. On the third day after the abscess opened pain supervened above the pubes, became acute, and in a few hours extended over the whole lower part of the abdomen laterally, and as high as the umbilicus. A blister was applied below the umbilicus, which entirely removed the pain and tenderness. Hectic was now fully developed, and the patient gradually sunk, exhausted by the profuse suppuration, night-sweats, and diarrhœa, and died in the middle of September, 1839. I was not permitted to examine the body.

This case of non-contagious (probably at its commencement subacute) inflammation of the mucous membrane of the vagina, urethra, and bladder, causing ulceration of the lining membrane of the latter organ, and the subsequent formation of abscess between its coats, the discharge from which eventually destroyed the patient, shows the danger of delay in opposing appropriate remedies to existing disease, the necessity of a correct diagnosis, and the difficulty, not to say the impossibility, of arriving at it without a careful examination of the seat of the malady. It likewise proves that urethral irritation, in connection with vaginal discharge, is not confined to cases of a contagious character.

I am, Sir, your obedient servant,

H. C. Roods.

67, Great Russell-street,
May 6, 1842.

DR. ROBERT LEE'S CLINICAL REPORTS IN MIDWIFERY.

No branch of medicine is so universally interesting to the faculty as the obstetric. The difficulties which continually beset practitioners in this department form to them a perpetual source of anxiety, and keep the mind alive to every useful hint that is derivable from experience; and the surgeon or physician who frankly communicates to the public the results of his experience, has a genuine claim to its gratitude. With this feeling we cordially award to Dr. Robert Lee our meed of approbation, for the concise, simple, and practical "clinical reports" in midwifery which he has lately given to the

public in a contemporary journal ("Medical Gazette").

The first report includes the history of twenty-three cases of uterine hæmorrhage with placenta presentations. In all, the nature of the presentation was without difficulty ascertained. Ten of the cases proved fatal. Six cases were complicated with rigid "*os uteri*," and one with great pelvic deformity. In six, uterine phlebitis occurred. With reference to these cases, Dr. Lee observes,—

"All the best writers on midwifery during the last hundred years have recommended artificial delivery where the placenta presents, and hæmorrhage takes place in the latter months of pregnancy. Every practitioner knows that the operation of turning should be performed in such cases, when the flooding becomes profuse, and the orifice of the uterus is dilatable. But in some cases the orifice remains so rigid as to render the introduction of the hand into the uterus impracticable, while a discharge of blood is taking place sufficiently great to endanger or destroy life. There are no cases more embarrassing to the accoucheur than these, and none in the practice of midwifery attended with greater hazard. That they are not uncommon, and that we possess no means of effectually controlling the hæmorrhage till the operation of turning can be safely performed, the following histories sufficiently prove."

The seven most interesting we condense as follows:—

1. A. C., ætat. 42, was far advanced in pregnancy, and had been attacked with profuse uterine hæmorrhage ten days before. The placenta was adherent to the neck of the uterus, but the orifice being rigid, the plug was employed for several days to check the hæmorrhage, till turning could be performed. A large quantity of blood notwithstanding was lost both before and after delivery, and the exhaustion was quite alarming. She survived for eighteen days, and death then took place from inflammation of the left spermatic vein, and gangrene of the left lung.

2. On the 26th of April, 1835, a patient was more than seven months pregnant, and had been attacked a fortnight before with profuse uterine hæmorrhage. The first discharge of blood took place in the night when she was at rest. It was preceded by a sense of uneasiness about the uterus. A considerable oozing of blood still continued when I first saw her. The placenta presented. The os tincæ was as large as a crown-piece, but its margin was so hard and undilatable, that I found it impossible, without employing too great force, to pass the hand into the uterus. After a cautious trial for about half an hour to get the hand insinuated through the orifice, I was compelled to withdraw it, as there was no hope of overcoming the resistance.

On the 27th the flow of blood continued. The strength remaining unimpaired, and the os uteri still unyielding, I resolved to wait till relaxation should take place, and to moderate the discharge by the recumbent position, and by the application of cold within and without.

On the 28th a large quantity of blood suddenly escaped, and produced syncope. The countenance was afterwards pale, the extremities cold, and the pulse rapid and feeble. The os uteri being now soft and dilatable, I immediately passed up the hand and delivered by turning. The child was born alive. The placenta was removed soon after, but although no further loss of blood occurred, she sank gradually, and died in a few days.

3. A patient who had completed the seventh month of pregnancy, had been attacked with uterine hæmorrhage three weeks before. A slight discharge of blood had continued during the whole of this period, but it had produced little effect upon the system until a few hours before I saw her, when several pints of blood were suddenly discharged, and her whole strength was nearly extinguished. The pulse was imperceptible, the extremities cold, and respiration feeble. The blood still continued to flow in great quantities, and it was evident death would soon take place if the uterus were not speedily emptied of its contents.

The os uteri was not dilated to the size of a crown, and it was so rigid that I found it absolutely impossible, though I employed a high degree of force, to push more than three fingers within it. The whole hand could not be made to pass, notwithstanding the urgency of the case. On the fingers being withdrawn for a short time, the flooding continued. I made another effort to turn the child, but could not overcome the resistance. I then pressed forward the fore and middle fingers of the right hand between the placenta and uterus, so as to reach the membranes, which I tore open. Pressing the fingers still forward, they came in contact with one of the feet, which they grasped, and brought down into the vagina. This was pulled lower and lower, till the whole extremity and nates were drawn into the os uteri; but so rigid did it continue to be, that although I exerted all the force I dared employ in dragging it down, half an hour elapsed before the pelvis of the child could be made to clear the orifice of the uterus: at last it was extracted with the placenta, and the hæmorrhage ceased.

A violent rigor followed, which threatened for a time to destroy the patient. Bottles of hot water were applied to the feet and pit of

the stomach, the whole body was covered with hot blankets, and brandy was liberally administered. She slowly recovered from the effects of this immense loss of blood.

4. Mrs. R., whose pelvis is greatly distorted by rickets, was attacked suddenly with profuse uterine hæmorrhage in the eighth month of pregnancy. I had delivered her once by craniotomy, and induced premature labour six times. She refused to submit to the operation on this occasion. On examination at four o'clock the following morning, a large portion of the placenta was felt detached and protruding through the os uteri. The orifice, though little dilated, was in a state to admit artificial delivery, but so great was the pelvic distortion that I found it impossible to introduce the hand within the pelvis to turn the child. The flooding continued. There were no labour pains. I could feel the head above the brim of the pelvis, and I determined to endeavour to open it and extract it with the *crotchet*. An assistant pressed hard over the fundus uteri, while I carried forward the fore and middle fingers of my left hand to the head, which I could scarcely touch. In the groove formed between these fingers the point of the *perforator* was conducted to the head, and pressed steadily through the integuments and bone, and then the blades were opened. The undilated state of the orifice rendered this difficult, but it was done without injury. The *crotchet* was then introduced into the opening in the skull, and the head dragged down, between the placenta and uterus, into the brim of the pelvis, where it stuck fast for a long time. The orifice of the uterus was still imperfectly dilated. After *four* hours very hard work we succeeded in getting the base of the skull through the brim into the cavity of the pelvis, and delivered.

The placenta was removed soon after the child, and no hæmorrhage followed. This woman recovered, and she has since had premature labour, induced twice at the end of the seventh month.

5. A woman, aged 40, was affected with alarming uterine hæmorrhage. I found the *os uteri* dilated to the size of a crown, and very rigid. The placenta partially detached, the membranes were distinctly felt, and the head presented above them. The strength not much impaired. The hæmorrhage returned, when three doses of the ergot of rye were given; but though pains were induced, the hæmorrhage continued. At four, p.m., I found the membranes entire. I drew the nail, like a saw for some time, over a portion of them, and at length the *liquor amnii* escaped in great quantity. The head of the child was seen pressed down between the placenta and the anterior portion of the cervix uteri. The labour was safely completed in an hour. The child was dead; and for several months a continuous sanguineous discharge from the uterus remained.

6. Case of profuse uterine hæmorrhage; face of the patient pale, hands cold, pulse rapid and feeble. The os uteri so much dilated, that the points of the four fingers and the thumb could be readily introduced therein; the circumference was not thin, but it was soft. The turning was easily done, and the child born alive. *I left the placenta for some time* in its situation, after the extraction of the child, to produce the effect of a plug; it was afterwards removed, when the uterus had contracted; and the patient got well quickly.

7. A lady, more than eight months pregnant, at twelve o'clock 10th of last January, suddenly lost a quart of blood. At four, p.m., the hæmorrhage continued: at seven, p.m., I saw her, the pulse was rapid and feeble; the upper part of the vagina, filled with a clot of blood, adherent to the os uteri. By displacing this, at the back I could distinctly feel the placenta adhering all round to the cervix, which was thick and rigid, and but little dilated: the hæmorrhage was so great, that death would soon take place, if delivery were not accomplished. I passed the right hand into the vagina, and insinuated my fingers between the uterus and placenta, at the back part, and reached the membranes; but the rigidity of the orifice was such, that though I employed force for a considerable time, I could not succeed in getting the whole hand into the uterus. I felt one of the feet through the membranes, grasped it, and brought it down into the vagina enveloped in the membranes, which then gave way. Nearly half an hour elapsed before the version could be completed; and when it was effected, the neck of the uterus grasped the neck of the child so firmly, that I had the greatest difficulty to extract the head, and not till I had made pressure for some time with the fingers, and dilated the orifice of the uterus. A great discharge of blood instantly followed; the placenta was removed, and every means employed to stop the hæmorrhage; but the breathing became hurried, the extremities cold, and she died within an hour after delivery.

(To be continued.)

ON THE OPERATION FOR

VARICOSE VEINS.

By Charles Clay, M.D., M.R.C.S., and Lect. on Med. Juris., &c., Manchester.

In your Number for May 14th, just issued, I observe a communication on the above subject, by Dr. E. Scratchley, of Clarendon-street, in which reference is made to my operations for varix, reported in your valuable

Journal during the months of July, August, October, and December, 1840, and July, 1841. Dr. Scratchley observes on the difference in the time of the sloughs falling out, as stated by me and M. Laugier; mine being in a much shorter time. On reference to my cases, as reported in THE LANCET, he will observe the plan I pursue is not (in respect to the application of the Vienna paste) strictly *that* of Laugier; inasmuch as the paste (after the vein has been effectually laid bare) is applied solely, and particularly, to the coats of the vein, by pressing the wedge of paste made very stiff on the vein through a small fissure in the lint, made by scalpel or scissors. By this plan it will be perceived the integuments are effectually preserved from the attack of the caustic, and the size of the slough very materially lessened: this, in my opinion, is the reason why it falls out so much sooner (a fact of no little importance in the treatment of such cases). Dr. Scratchley thinks some discrepancy might arise from the state of the invalid when discharged. Now, although the cases I reported were discharged from active attendance at the expiration of the time specified, yet I did not lose sight of them for some time after; and it is with pleasure I can *now* add, all the cases up to this time continue to be free from varix, with the exception of two cases (out of fourteen) mentioned in my communication to THE LANCET of July 24, 1841: one of which has been operated on since, and is now quite well; the other case having removed from the neighbourhood, I have no means of ascertaining its progress. Such has been the result of fourteen cases, including about fifty incisions, without any symptoms of phlebitis occurring. This fact, together with the success of M. Laugier, induces me to repeat the good opinion I expressed on the subject early in 1840, and as earnestly to recommend it to the profession generally. It will be seen by my own cases, the principle of reducing the slough to the smallest size possible, is nearer the plan of M. Laugier than Berard: as to the opinion of the latter, in allowing his patients to return to their employment in three or four days after the operation, is to me so absurd, and fraught with danger, that I am inclined to think the printer must have mis-stated his meaning; I am quite convinced, and that from no little experience, that no case, however favourable, would be fit for exertion in that time, even in a very moderate degree: and what are we then to think of Berard, when he states, exertion, "*however laborious, may be followed in that time.*" I think, too, that about that time there is great danger of hæmorrhage, from premature separation of the slough, a circumstance quite within the range of possibility, by exertion alone. As all varix cases are indicative of constitutional debility, I should rather advise such cases to be kept from exertion as long as possible, and certainly not to return to it for at least three or four weeks, under the most favourable circumstances; and that bandages be kept on the parts for a considerable time after.

I cannot see the advantage of applying the caustic for one or two inches along the course of the vein, as Berard proposes. The perfect obliteration of the vein is all that is wanted, and that is as effectually accomplished in half an inch as half a yard. I have seen no case where the vein has not been obliterated, but have witnessed one in the hands of a friend of mine, where, in consequence of a portion of the integuments having been left over the vein, the caustic did not effectually destroy it, until I repeated the operation. Thus Berard's plan is liable to the same objection; and I think if he were to repeat the operation on his plan, very often he would find this to be the case.

The smaller the slough the better: there is no doubt, if the operation be properly performed, the vein will be effectually obliterated; and the excitement from a small destruction of parts, and that confined as much as possible to one tissue, there is less liability to phlebitis.

Cauterisation is an old practice, and has long been dreaded by practitioners, as well as ligatures, from the great tendency to phlebitis. Laugier (and not Berard) had the merit of reviving the operation in its present ameliorated form; and so little danger is there, if proper attention is observed, of phlebitis occurring, that I hope to see it practised more generally, when I am confident the plan of myself and M. Laugier will be found not in the slightest degree over-rated. There appears to me to be a much greater probability of bringing on phlebitis, when the vein is not sufficiently destroyed, if the solution of continuity in the venous tissue be perfectly and rapidly accomplished, scarcely a symptom of that dangerous affection can occur. All the cases reported by me were females, and most of them extreme cases. Dr. Scratchley will also observe, that I do not use near so much of the paste as M. Laugier; and that which is not in contact with the vein is so well protected by lint, that there is not the slightest necessity for the removal of the unemployed portion till next day. I prefer this, as the very motion, caused by clearing it away so early as half an hour or forty minutes after the operation, might excite hæmorrhage. I may also add, that by waiting a few moments after the vein is laid bare, the bleeding immediately subsides: thus there is no call for the nitrate of silver to be applied to the integuments to check bleeding, as used by Laugier; by this plan much pain and subsequent irritation is avoided.

MEDICAL REASONING.

To the Editor of THE LANCET.

SIR,—I feel strongly induced to point out to your notice a fine specimen of the inconclusive and behindhand style of medical reasoning, which the public, in their dogged ignorance of medicine, invariably prefer with the view of satisfying *their* curiosity, to all really just and scientific discoveries. I allude to a paragraph on gout which appeared in the *Times* of to-day (taken from a popular work on pendulous stomach), and which denounces as hopeless and useless the attempt to give other explanation of the subject.

I will first give the paragraph, with a few interverbal remarks; and afterwards advert very briefly to the real knowledge of gout and its treatment, which the profession have entertained now for some years, through the aid of chemical demonstration.

"GOUT.—There is probably no disease which has led to so much learned disquisition, from the time of Hippocrates to the present day."—[*Learned disquisition in the common acceptation is here of no use; we gain nothing in medicine or chemistry by searching the ancients.*]—"By some persons I may be censured for temerity, but having looked at the complaint for many years, uninfluenced by theories and speculations,"—[*chemical or otherwise?*]—"having narrowly watched its visitations, progress, and end, in the personal sufferings of my own father, unswayed by reasonings on a wrong basis, wherein effects are treated as causes, and causes as effects,"—[*perhaps unswayed by any reasoning whatever, and only by simple observation,*]—"I have never been able to separate from my conviction the simple fact that gout under all its forms is essentially the same thing, and invariably accompanied with a disordered state of the digestive organs."—[*Certainly it is one effect of disordered digestion; yet indigestion is not always followed by gout.*]—"It is so far constitutional that its attacks are always favoured by a certain condition of the mucous membrane of the stomach, original or induced."—[*What is this condition?*]—"All its premonitory symptoms are those of indigestion"—[*true*];—"and however mild or aggravated, however various and multitudinous, incipient or confirmed, they still resolve themselves into one principle of action, the inflammatory,—into one chief seat,—the stomach. But they may differ in the manner in which they are developed, as much as the faces of those differ in whom the disease is found. It is hopeless, and one might almost say useless, to attempt any other definition.

These indefinite and inconclusive observations are reviewed by the *Times* newspaper as the ultimate effort of science upon the nature of gout. Surely the true theory, which can be demonstrated by the aid of chemistry, would be quite as intelligible to the world at large as the above paragraph, which says only that gout is indigestion, and that indigestion is gout: and it would have this additional advantage,—it would convey correct information.

Pathology of Rheumatism, Gout, and Uric Gravel.

These three diseases are all produced by the same essential cause, and that is, an acid digestion, or rather, perhaps, *indigestion*, of stomach. For this acidity displaces an animal base called urea, either in the course of the general circulation of the body, producing rheumatism or gout (rheumatism if it occur in the muscles, or the larger joints; gout if in the smaller joints), or in the kidneys, in the form of gravel. In the case of rheumatism and gout the urea is in combination with soda, forming urate of soda, which is an insoluble salt. Thus urate of soda is the cause of rheumatism and gout; whilst urea itself, when deposited in the kidneys, constitutes gravel. And it is owing to acidity from indigestion that urea is deposited either in the kidney or in the general circulation of the body, where it will be free to combine with the pervading alkali—soda, and thus produce gout or rheumatism.

The theory of the scientific treatment of these diseases is therefore as follows:—In rheumatism or gout the insoluble urate of soda may be decomposed by the continued exhibition of potass; the urate of potass being a soluble salt. Now, also, if the patient's urine be chemically examined before and after taking colchicum, in the latter case it will be found that a much larger quantity of urea is excreted. Thus, by the exhibition of potass and colchicum, the urate of soda is not only decomposed, but also urea is expelled from the system. Beyond this it is only necessary to consider that the deposit of the insoluble salt—urate of soda in rheumatism and gout—may locally obstruct the capillary circulation, and so, more or less, cause inflammation: the treatment of which, in addition to the radical treatment, will require sedatives or depletion. In the case of gravel, the colchicum is not advisable, as it would add to the complaint; that is, it would increase the quantity of urea in the kidney. All that is necessary is, the continued use of alkaline medicines, with sedatives to allay irritability and pain: and further, as in the case of rheumatism and gout, the most radical object is to prevent the acidity of stomach which causes these diseases; and this is only to be done by correcting and improving the digestive powers. But to attempt this latter plan alone during the paroxysm of any of these diseases, would give no relief whatever. I remain, Sir, yours, &c.,

R. S.

Kennington, May 16, 1842.

THE BRAIN.—SORE NIPPLE.

To the Editor of THE LANCET.

SIR,—I forward the following cases for insertion in your valuable Journal, and remain your obedient servant,

R. DAVIS, Surgeon.

1, Worship-square, Finsbury,
May 10, 1842.

Mr. B., a married man, forty-five years of age, of plethoric habit, florid countenance, light brown hair, blue eyes, and of the middle stature, was seized, about eighteen months ago, with an attack of paralysis, under rather peculiar circumstances. Being of the sanguineous temperament, he has been a man of warm passions, very fond of women, sexual intercourse being his besetting sin. He has a short, thick neck, and a tolerably good development of the occipital region; he has no disease of either heart or lungs, nor had he ever suffered from any particular illness previous to his late attack. He states, that about eighteen months ago while walking in company with a friend, and without much previous warning, he was seized with a fit in the street. A stick which he held in his hand suddenly fell down, and, leaving his friend, he was propelled forwards for several yards by a power over which he had no control; his friend, surprised at the strangeness and suddenness of his movement, getting in front of him, arrested his further progress. He was then taken home, where he immediately became insensible, and so continued for eight hours. Before consciousness returned he was bled, had mustard plasters to the feet, and all the remedies applied which are usual in such cases.

Paralysis of the whole of the right side was the consequence of this attack. In the course of four or five weeks he so far recovered his muscular strength as to be able to walk about. He is now under my care; his present symptoms are stammering and loss of memory; the treatment consists in the administration of an occasional dose of calomel at night, and a dose of some saline aperient in the morning; cold effusion on the head, and bathing the feet in hot water. The cause of the paralysis is evident, rupture of a blood-vessel, and the gradual effusion of blood into some portion of the brain; but the question is, Into what part did the extravasation take place? Did the extravasation take place in the cerebellum? The habits of the man would incline us to think so, but Magendie and Andral, who attribute the regulation of muscular action in a particular direction to it, deny it: they say that lesions of the cerebellum cause the patient to walk backwards. The effusion, then, must have taken place in the corpora striata; which bodies, according to the foregoing authorities, regulate motion forwards. The point of interest in this case is, what practical illustration does it afford of the truth of Magendie's experiments, which assign motion in different directions to effects produced on different portions of the brain.

Treatment of Sore Nipple.—Having read a short account of the treatment of sore nipple in THE LANCET, and as its cure is a great desideratum, I beg to state one case out of many. Mrs. W., residing in Primrose-street, Bishopsgate, suffered very much from this troublesome affection after her last confinement, not having had a child for some years previously. The summit of each nipple was completely sloughed off, and such was her pain whenever she gave the infant the breast, that she contemplated weaning it. I had resorted to several remedies for her relief; such as a shield nipple, bathing the parts with diluted sulphuric acid, and tincture of myrrh. The last remedy applied, and that which proved successful, was the lunar caustic, in substance. The surface of the sore was touched with it every morning, and the nipple was then covered with a piece of wet lint; the child was not allowed to suck the nipple under treatment, being supported from the other breast, the draught of milk in the unused breast being excited by friction. Under this treatment both nipples were healed in a comparatively short time.

DEFENCE OF TOTAL ABSTINENCE FROM ALCOHOLIC DRINKS.

To the Editor of THE LANCET.

SIR,—In THE LANCET of the 16th instant you favoured your readers with an extract from Dr. Clutterbuck's account of Dr. Birkbeck, bearing on the subject of total abstinence and temperance, upon which I beg to offer a few remarks.

"A rigid abstinence," observes the doctor, "in regard to either food or drink, is not, generally speaking, advisable." That a rigid abstinence with regard to food is, generally speaking, anything but advisable, every person in his senses will admit, and that a like abstinence from wholesome drinks would be equally absurd, I am quite ready to concede. But the question is, Are intoxicating drinks *good* for man in a state of health? I believe that they are not; and if chemical and physiological facts, and the opinions of medical men of the very first eminence in all countries, and in all ages, and the experience of millions of total abstainers, are at all to be relied upon, they are anything but in favour of the doctor's *observation and experience*.

That "of a generous diet both with respect to food and drinks, with the addition of arti-

ficial clothing, is not only good as a preventive, but good in the treatment of many of the diseases of our variable climate," I freely admit; but I have yet to learn that there is anything generous, anything invigorating, in even the temperate use (a very improper expression when applied to alcoholic drinks) of intoxicating liquors. Dr. Elliotson, a man in no respect inferior to Dr. C., says, in his excellent work on Human Physiology, "The total abstinence from alcoholic drinks and other narcotic substitutes, will greatly *augment* health and lengthen life." Dr. Kaye Greville in his evidence before the Parliamentary Committee observes, "It is my deliberate opinion that the use of intoxicating liquors is unnecessary to the healthy human constitution, and the strength which they seem to impart is temporary and unnatural. It is a *present* injury purchased at the expense of *future* weakness." The reason which Dr. C. gives for using these liquors, namely, the variableness of our climate and our artificial habits, is in my mind a strong one in favour of total abstinence from them. Any amount of testimony could be adduced to show that the use of such liquors renders the body not only more susceptible of atmospherical vicissitudes, but our artificial habits more destructive of health; and that, on the contrary, total abstinence disarms both of many of their otherwise dangerous effects.

Temperance is not simply another word for *moderation*, as the doctor seems to suppose. If he will examine the writings of some of our ablest Bible commentators, he will find that they consider temperance to consist in total abstinence from every bad thing (as beer, wine, and spirits), and moderation in every good. The doctor's "comfortable doctrine," as he terms it, is such to all lovers of a "short life and a merry one;" but those who desire length of days and the enjoyment of true happiness, will do well to consider the following weighty sentence of the celebrated Boerhaave, "Food, not too fat and gross, and water as a drink, render our bodies the most firm and strong."

Teetotalers no more denounce as simple every or anything in the shape of enjoyment than does the worthy doctor; but, while he thinks that there is to be found enjoyment over the bottle, they believe that the use of intoxicating liquors is their abuse, and that they cannot be enjoyed but at the expense of the constitution. If the doctor would attend but one temperance festival, he would never again bestow the epithet "ascetics" or total abstainers; he would there see *true* enjoyment, "the feast of reason and the flow of soul," in reality. I am, Sir, yours respectfully,

A. Courtney, Surgeon R.N.

Ramsgate, April 25, 1842.

REMARKS MADE IN OPPOSITION TO THE

VIEWS OF DR. CLUTTERBUCK

ON

ABSTINENCE FROM SPIRITUOUS LIQUORS.

To the Editor of The Lancet.

Sir,—I feel that I should ill discharge the duty which I owe no less to the claims of humanity than to the interests of science, if I omitted to notice an article which has appeared in The Lancet of the 16th instant, and headed, "Dr. Clutterbuck on Total Abstinence and Temperance." That Dr. Clutterbuck should have selected the character of his early friend and former colleague as the subject of a paper recently read by him before the Medical Society of London, and of which he was for some years the able president, can excite no surprise, since that lamented individual possessed, in a high degree, those moral and intellectual endowments which marked him out as a most fitting object of public eulogium. I will not conceal from you, however, the disappointment which I felt on observing the loose manner in which Dr. Clutterbuck spoke of "total abstinence;" and more especially as, from his high reputation both as an author and a lecturer, besides being a physician of "such extensive experience," a high degree of deference will be paid to his opinion on a subject "so much agitated" and so vastly momentous. Now, when it is recollected that tens of thousands of valuable lives are annually sacrificed to the use of intoxicating drinks, it is not too much to aver that their influence upon the human system deserves the most calm and scientific investigation; at any rate, the dreadful havoc which is made on society by their general employment, is calculated to urge a deep and earnest inquiry how far they are *absolutely necessary for the sustenance of man*. This is, therefore, a question of intense interest, and ought not to be approached with indifference, or rejected with disdain; nor ought this great moral and physiological inquiry to be considered as coming only within the province of "temperance societies," since it involves considerations of the highest interest to the human family, and considerations which force themselves daily upon the attention of every medical man; and whether we are desirous of evading the question or not, it is quite impossible to avoid the direct responsibility which is entailed upon every member of the healing art upon a subject so closely allied to the public health. Sir Astley Cooper in speaking on this subject to a friend only a few months before his death said, "on this subject we have all been most sadly deceived;" and well he might, for on no subject has the public mind been so thoroughly abused as on the *nature and pro-*

perties of alcoholic drinks. All classes have been equally misled; nor have medical men been suffered to escape the "great delusion." There has, however, within the last few years been instituted, chiefly, it must be admitted, through the influence of "temperance societies," such a general agitation of this subject, that few are now disposed to concede to intoxicating drinks those salutary attributes which they were wont to possess; and even medical men, who appear almost the last to exhibit any sympathy with the "temperance" movement, are more indebted to this source for sound and rational views on this subject than to the recognised principles of medical science: not only have the phenomena resulting from intemperance been more clearly ascertained, but it has been suggested, and many enlightened and acute pathologists believed, that intoxicating liquors taken in "moderation" are injurious to the human frame; at any rate, it has been triumphantly demonstrated, that all kinds of intoxicating drinks may be dispensed with, and without any injury to the constitution; nay more, that the practice of "total abstinence" is, in a very high degree, salutary to the system; and if arguments were wanting to prove the efficiency and advantages of "teetotalism," they are furnished by millions in the individual experience of every abstainer from intoxicating drinks! Time was when this system was deemed to be utopian and absurd; and I confess myself to have been at one period as sceptical on this subject as any of my brethren: nearly seven years' experience in my own person, however, and daily observation of those around me, have served to convince me of the truth and validity of these principles; and without committing myself to the dogmas of the hydropathists, I may be allowed to say that there is no department of medicine of more interest or of higher importance than a due consideration of those diseases which are directly or indirectly the result of *alcoholic agency;* for not only are there those glaring and more obvious symptoms which are familiar to the most superficial observer, but other and not less important affections which, although more occult and insidious in their influence, are equally fatal in their results, and which are too often unsuspected as to their real origin.

The time has gone by when any man can hope to succeed in upholding the former reputation of alcoholic drinks already tottering to its fall; and it must be vain for Dr. Clutterbuck to expect that he shall be able successfully to repudiate the principles of "total abstinence." It surely can be no sufficient argument to say that intoxicating drinks are necessary, because "ours is in a great degree a state of artificial existence." Do not most of our domesticated animals maintain an equally "artificial existence?" But Dr. Clutterbuck very wisely observes, that "experience in these matters ought to be our chief and only guide;" adding also, that "diseases, peculiarly incident to a cold and variable climate like ours, such as, for instance, pulmonary diseases and scrofula in all its variety of forms (which, together, constitute the great mass of our diseases), are most effectually prevented by what is called a *generous diet*, both in respect to *food and drinks;* while in the treatment of diseases of this class the same general principles ought to be kept in view." With regard to "pulmonary diseases," there can be no doubt but that they are often superinduced by the use of intoxicating liquors; and highly as I approve of a "generous diet" in cases of scrofula in "almost all its variety of forms," I do not think that, in order to constitute a "generous diet," it is necessary to employ *intoxicating drinks.* That system of diet must be the most "generous" which is the most nutritive; and as intoxicating liquors contain only a very limited supply of nutriment, and some none at all, I am led to the conclusion that "nature" is under less obligation to intoxicating drinks than Dr. Clutterbuck would have us to believe. It is well known that *ardent spirits* possess no single property of nutrition. *Wines* for the most part but ill deserve the reputation which they have so universally acquired; few indeed can be relied upon as genuine; and many are "vile brandied compounds," imposed on society under the names of port, sherry, Madeira, &c. Franklin spoke only the truth when he asserted that a penny loaf contained more nourishment than a gallon of *ale;* nor will it be possible much longer to compound and synonymise *stimulation* and *strength!*

The great almoners of health are, wholesome food, pure air, moderate exercise, sound sleep, and good water. The most "generous diet" may be supplied without a single drop of intoxicating liquor;" and let it be recollected that alcohol, which constitutes the specific character of intoxicating drinks, is the fruitful parent of innumerable diseases, and so far from possessing wholesome and salutary properties even in those prophylactic combinations in which it is said to be disarmed of its deleterious properties, it is the same noxious and pernicious article. In the stomach it undergoes no change, but passes into the circulation without any mitigation of its character; it carbonises the blood more rapidly than it otherwise would be; it enters the delicate and sensitive organs and tissues of the body, and in many ways lights up disease as palpably as it excites the brain to congestive inflammation and delirium. If we had not the advantage of direct "experience" on the subject, common sense and sound philosophy would suggest that the great wear and tear of life is occasioned by the exhausting influence of moral and physical excitement; "who would add

momentum to an avalanche?" And is it not clear that to add the stimulus of intoxicating drink to the ordinary stimulus of arduous mental or bodily toil, is "lighting the candle at both ends!" Nor is it easy to admit the force of Dr. Clutterbuck's opinion, that intoxicating liquors "are required by the variable nature of our climate;" and here I would adduce not my own experience only, but that also of many others, whose occupations have exposed them to every vicissitude of this variable climate, and who have assured me that since they adopted the "total abstinence" principle, they have enjoyed a greater immunity from disorders and a more firm and robust health than ever they did previously.

Can Dr. Clutterbuck be insensible to the fact, that there are at this day living within the kingdom of Great Britain upwards of seven millions of total abstainers from all intoxicating drinks? Surely numbers cannot be wanting to prove the validity of the principles! And of these, are persons of all *ranks*, from the peer to the peasant; of all constitutions, from the athletic to the effeminate; of all occupations, from the laborious artisan to the sedentary employée; of all *ages*, from the infant at the breast to the veteran of ninety; and of all parts of the kingdom, from "John o'Groats" to the "Land's-end." So that so far as "experience" goes, it is all on the side of total abstinence.

Children nursed on total abstinence principles escape many of the disorders so common to childhood. Females who abstain from alcoholic drinks enjoy, during pregnancy, an immunity from many distressing symptoms incident to this interesting period. Mothers who "abstain" prove, during lactation, the utter fallacy of those vulgar prejudices which assume the necessity for alcoholic drinks. Hereditary diseases, which are so common, more particularly *scurvy* and *scrofula*, are greatly mitigated, if not wholly destroyed, upon this plan. Convulsions, to which nurselings are so liable, and which are frequently supposed to require the gum lance, are too frequently caused by the alcoholic milk. During the adolescent period, when the rising generation is too commonly initiated into the use and relish of intoxicating liquors, "total abstinence" is of the highest importance, not only as preventive of a dangerous appetite, but also as tending to promote a sound and healthy state of the system at a period when the seeds of a premature decay are often sown by an indulgence in such liquors. Total abstinence is singularly serviceable in placing the constitution in a state favourable to an exemption from diseases; whilst those induced by a contrary practice are numerous and formidable. The ordinary headachs which follow alcoholic potations prove the peculiar sensibility of the brain to their morbid influence; and the usual phenomena which attend and follow a fit of drunkenness exhibit, in a striking manner, the influence of this excitement. Many of the nervous class of disorders are the certain fruit of spirituous excitement; whilst the most aggravated forms of apoplexy, paralysis, epilepsy, and mania, are amongst the progeny of this prolific parent. Nor is it surprising that a system of diet, which is so detrimental to the healthy functions of the heart, the brain, the organs of respiration and of nutrition, should induce a deteriorating influence upon the animal spirits, as well as the physical energies of the whole system; and startling as the opinion may be, it is scarcely too much to assert, that human nature may be sooner worn down by intoxicating drinks than worn out by hard labour; and it may be worthy of inquiry, whether there are not more deaths from the effects of moderate drinking than victims to intemperance: at any rate, no man who indulges in intoxicating drinks can say what mischief may not ensue, or to what extent it may not proceed. I am aware, however, that wines and other alcoholic drinks are employed in the treatment of diseases by many eminent physicians; and if reliance is to be placed on their statements on this head, with actual advantage. I am bound to state, however, that in cases strictly analogous to those referred to, I have known equal success without anything of the sort, and I strongly incline to the belief, that I have frequently administered these remedies with disadvantage to my patients, and I greatly fear that thousands have been sent prematurely to the grave through the injudicious administration of alcoholic stimulants; at the same time, I can most readily believe that many recoveries have been protracted, if not prevented, by seizing the first opportunity that has presented itself on the subsidence of the more active symptoms, for commencing a course of stimulation, by which, in many cases, the dying embers of disease have again been lighted up, and secondary symptoms have been established, which have either terminated in death or in a sequela of difficult and uncertain removal. In convalescence, after fevers and other active diseases, I have generally found that a light and nutritious diet has proved more serviceable than a contrary practice. Cases are continually occurring wherein the use of wines or malt liquor, more especially porter, are employed as tonics, and of this class the latter is quite a favourite! And in how many cases have they proved worse than useless? not unfrequently aggravating the symptoms for which they were prescribed! Dr. Clutterbuck cites the case of his deceased friend, which proves as strongly as anything can do the value of the principle for which I contend; and yet by way of corollary, in alluding to the *abstemious habits* of Dr. Birkbeck, he takes occasion to assail the system of total

abstinence! "And this," he says, "I am induced to do for the purpose of remarking, that a rigid abstinence, in regard to either food or drink, is not, generally speaking, advisable;" adding, "It is no argument to say that intoxicating drinks are unnatural, and therefore injurious to the human frame. Nature does not supply us with adequate or proper food, unless herself stimulated by artificial means. There seems, therefore, to be no reason (*à priori* at least) for abstaining altogether from artificial excitement." And does Dr. Clutterbuck think such reasoning as this can vindicate the necessity for *alcoholic* drinks? He must take a limited, not to say a mistaken, view of the matter, if he supposes that abstinence from spirituous liquors implies or involves a "rigid abstinence" with regard to food. No such thing. Nor is the proper excitement and really healthy stimulus connected with good and wholesome food to be confounded, any more than it is to be compared with the morbid excitement of alcohol. It is equally superfluous for Dr. Clutterbuck to exclaim against the "ascetic" character of those who refuse to take intoxicating drinks, since we know such to be amongst the healthiest and the happiest of our species; nor can it avail anything to allude to individuals "who live to a great age, with an extraordinary exemption from disease, whose habits of life are far from temperate." Of such instances of tenacity of life, there can be but one opinion—they are rare *exceptions* to the general rule! Nor would Dr. Clutterbuck or any other physician risk his reputation on maintaining that those who "are far from temperate live to a great age, with an extraordinary exemption from disease." But Dr. Clutterbuck adds, "for myself, at least, I confess I am not of the *ascetic* tribe of philosophers who denounce as sinful everything in the shape of *enjoyment*, and who inculcate the notion that the only path to heaven is strewed with thorns." This, at least, is sufficiently intelligible; and after such a confession there need be "no mistake" as to the *animus* by which Dr. Clutterbuck is influenced in his opposition to total abstinence.

——"Non invideo,
Non tali auxilio, nec istis defensoribus,
Sed majis minor!"

I am, Sir, your obedient servant,
T. Beaumont.

Bradford, Yorkshire, April 28, 1842.

"It is the most common of all possible whims, to represent studious persons as not only exposed to all sorts of disorders, but as very short lived. But the records of academies, as well remarked by Alibert, sufficiently demonstrate that there are few classes of men who live to a greater age than philosophers."—*Quarterly Review.*

THE LANCET.

London, Saturday, June 4, 1842.

"The remuneration of medical officers *should be* such as to insure PROPER ATTENTION and the BEST MEDICINES."—*Parliamentary Committee.*

"The guardians will doubtless perceive that *unless* the medical officer be *adequately* REMUNERATED, no vigilance on their part will suffice to secure PROPER ATTENDANCE to the POOR under his care."—*Poor-law Commissioners.*

The principle here asserted is so self-evident, that it scarcely needs the authority of the Parliamentary Committee, or the Poor-law Commissioners, for its support; but we are led to repeat it by the astounding fact, that notwithstanding the report of the Parliamentary Committee, the salaries of the medical officers under the law administered by the Poor-law Commissioners who penned the above sentence, still amounts to no more than

One shilling and fivepence farthing a case for medicines and attendance in the metropolis;

And *three shillings and threepence* a case for medicines, advice, and journeys, in the wide country districts;

while the cheapest genuine medicines cost at the wholesale price *two shillings and sixpence* a case. For the supply of 9233 out-patients and 4025 in-patients treated at St. George's Hospital in two years, the medicines cost 276*l.*, or nearly *four shillings and twopence* a case, exclusive of 73*l.* paid for surgical instruments, spirits of wine, brandy, vinegar, and chloride of lime. The drugs and medical appliances for 66,191 patients (49,395 out-patients, 16,796 in-patients), cost in twenty-one hospitals, according to Mr. OXENDEN's return, 15,216*l.*, or *four shillings and sevenpence* on each case: wine was included, which will partly account for the difference.

At nine county hospitals from which returns were given in the "British Medical Almanac" (1837), the medicines for 38,428 patients cost 6898*l.*, or *three shillings and sixpence halfpenny a case*, exclusive of wine and spirits.

Are these facts disputed? They were published in the Report of the Parliamentary Committee, and their accuracy has been admitted on all hands. Is the sum unreasonable? Why, four *draughts* which a patient takes in a day would cost *four shillings* at the druggist's shop! The sum which is paid under the new Poor-law for attending a case of sickness, and for the medicines with which the poor man is supplied, does not amount to so much as the rich man's draughts of one day cost at the druggist's price. This is the remuneration of the medical officers. How different from what the Parliamentary Committee declared it "SHOULD BE," and from the "adequate remuneration" recommended by the Poor-law Commissioners themselves in their late reports!

If we chose to give individual cases of the deplorable consequences to the poor of the inadequate remuneration of their medical attendants, we have many at hand; and there would be equal facility in bringing the blame home to the doors of the first Poor-law Commissioners, who acted with the utmost brutality towards the medical profession, pushed the tender system to its utmost bent, propagated all kinds of falsehood respecting the rates of remuneration, and appeared to consider that it was their particular mission to cut down the population in every possible way, but especially by depriving the paupers of efficient medical attendance. We shall not, however, at present adduce single cases, nor attempt to excite the feelings by tracing the history of a controversy so painful, as two of the first Commissioners have resigned, and their doctrines have now been candidly abandoned, in theory, by Mr. CORNEWALL LEWIS and Sir EDMUND HEAD.

It will be sufficient here to state briefly the principles which should regulate the remuneration of the medical officers, and the administration of medical relief under the Poor-law. In the first place, the medical attendance provided by the public for the sick poor should be as good as can be obtained; the remedies with which the poor are supplied should be genuine and efficient, the medical officers should possess the ordinary average skill, devote time enough to form an accurate diagnosis of the cases of the poor, and watch the progress and changes of their diseases carefully, in order to adapt the treatment to every exigency as it may arise. Unless the medical attendance be of this quality, it were better for all parties that it should be altogether refused under the law; for the character of the medical profession would not then be compromised, the poor would not be deceived, and the rate-payers would not be burthened with the maimed victims of bad physic and bad surgery, which every person of the least observation is well aware commonly thwart the efforts of nature, and are worse than no physic or surgery at all.

To secure efficient medical attendance the medical officers must be adequately remunerated. We demand it as their right. That they deserve it, where they are efficient, every candid person must admit, who is acquainted with the nature of the parochial medical duties, the visits to the most miserable abodes, the exposure to infectious diseases, the disturbances at all hours of the night, and the patient attention to the feelings and prejudices of the ignorant, unfortunate, and wretched.

The consequences to the poor and the parish of inadequate rates of remuneration, such as those now given in the majority of unions, were described to the Parliamentary Committee with painful accuracy by the secretary to the Poor-law Committee of the Provincial Association, who had too good grounds for his statements.

"14907. Mr. WAKLEY.] If the medical officers are not satisfied with their remuneration, do you conceive that by a very rigid superintendence of them they may be made to discharge their duty so effectually that

the poor would be equally well off as though the medical men were perfectly satisfied?—No; I am quite convinced that the medical men have the means of deceiving both their patients and their employers on that point.

"14908. Then you are decidedly of opinion that if the arrangements with the medical practitioner are of a character to produce dissatisfaction in his mind, the interests of the poor must suffer in consequence?—It is inevitable, except the medical man is a man of extreme humanity, and there are many such in the profession; but *you cannot offer a continual temptation to men to do what is wrong, and not find many who will fall under the power of that temptation.*

"14909. Is it in the power of a medical practitioner to deceive non-professional persons, with regard to the propriety of his attendance on the patients?—Most undoubtedly it is.

"14910. Might he not, to a great extent, neglect his duties without an unprofessional man being able to detect it?—Yes.

"14911. Thus might not the poor suffer very much, not only in agony of body and of mind, but also in reference to their pecuniary interests?—Yes, they might.

"14912. Might not a labouring man, for example, through *inefficient attendance, become permanently disabled*, and consequently be a charge upon the funds of the union during the remainder of his life?—*Yes, I have no doubt that that has happened.*

"14913. Do not you consider that strict, prudent economy ought to induce the guardians in all instances to provide efficient medical attendance for the poor?—Clearly so.

"14914. And that that is, after all, the strictest economy?—Yes."

It is quite true that young men, for the sake of an introduction to practice, and to gain practical experience, may be induced to supply the poor with remedies out of their own pockets, at a considerable sacrifice, and to devote to the cases of the poor time and attention which they could employ in no other way. But they would not do this for many years: they must either acquire a remunerating practice or be ruined, unless they have an independent fortune, in which case it is not very likely that they would be union medical officers. Then, a succession of young practitioners could not be obtained every two or three years in any district, and the Poor-law Commissioners, in their new order, contemplate the permanent possession of office by the medical officer, as the office is to be held during good behaviour.

It was found in 1837, that the proportion of persons who died under the care of the Poor-law medical officers was one-fifth part of the total deaths, or about 67,000 annually, which would, according to the same returns, imply, if the mortality was one in forty, that 2,680,000 are attended by the parish medical officers. Now, it is too much to expect medical men to supply 2,680,000 of their fellow-countrymen with gratuitous professional advice, and to contribute their labour in so unreasonable a proportion to the rest of the rate-payers; yet it would appear that in the aggregate, although there are exceptions in the present unequal system, the medical officers not only supply the paupers under their care with advice gratuitously, but with a considerable share of the medicines required (where the medicines are supplied at all); for if there be 67,000 deaths, there must be about 87,000 sick paupers constantly under treatment, the supply of medicines for whom, according to the hospital returns, would amount to from 261,000*l.* to 435,000*l.* at the cost price. But the Poor-law Commissioners tell us that the whole salaries and fees of the Poor-law medical officers only amount to 154,000*l.*

If a Central Commission were not to be maintained, it would still be the duty of Government to see that the administration of medical attendance on the poor was efficient, and was not conducted on vicious principles; to see, for instance, that the salaries were not, as they are now, so low as to deprive the medical officer of the means of supplying his patients with efficient remedies. If the Central Board of the Commission be continued, we shall be prepared to prove that the medical officers of the poor should be organised on the same general principles as the medical officers of the army, and be paid salaries which would amount on an average to 3*s.* or 4*s.* a-head on the pauper population. A medical officer who was acquainted with the subject might with the Commissioners adjust the salaries and districts in the present year, and have all the arrangements ready by next year. No-

thing less than this can be satisfactory. The late order of the Commissioners reminds us of a trick among children, who present their friends with walnuts containing everything that they should hold excepting the kernel; or egg-shells, which look very well outside, but embrace neither white nor yelk within.

WORKING OF THE NEW POOR-LAW MEDICAL ARRANGEMENTS.

To the Editor of THE LANCET.

SIR,—As you lately, in one of your leaders, admonished us to watch the proceedings of the different boards of guardians, I would accede to your desire, by reporting the proceedings of the Tendring board of guardians, which have been the first to come before the public in this quarter, under the new regulations of the poor-law commissioners.

In the last week's *Essex Standard* appeared an advertisement from the Tendring Union, mentioning the different districts, with the salaries attached to each; I need not occupy your valuable pages with the particulars of this precious document, but shall merely in broad terms state that the hundred of Tendring is a rich and extensive district, containing a population found at the last census to amount to 25,827 persons; and can it be credited that this most *liberal* board of guardians have offered the handsome sum of 260*l.* for drugs and medical attendance in that hundred, which is rather more than twopence farthing per head on the gross population!

In a neighbouring district with which I am acquainted, out of a population of about 2000, the medical officer reported in his register of illness 126 cases of pauper sickness, and at the same ratio in this population of 25,827, we shall probably have 1627 cases, which is threepence-halfpenny per case. I believe that in a report from the different hospitals and dispensaries that the average cost of each patient for *drugs* alone was stated to be three shillings. Thus we see that the Tendring gentlemen allow twopence to the medical practitioner for his valuable time and attention, and these cases average from seven to fourteen days in continuance. Such is the estimate that the farmers of Tendring have formed of the talents of their medical friends.

To the credit of the majority of the medical officers, I am happy to say that they have magnanimously rejected such a miserable remuneration; but it grieves us to add, that *all* have not acted in the same manner, for some, disgracing their profession, have accepted of this paltry salary; and it has been whispered to us that even in Colchester men are to be found ready to apply for more than one of the districts which are now vacant from the upright conduct of the resident medical men, who are nobly endeavouring to keep up the respectability of the profession.

Is this the practical working of the new regulations, which were to confer so great a boon on the medical officers of unions? If Mr. Commissioner Lewis be as great a friend to the medical profession as the president of the College of Surgeons states him to be, how can he sanction such outrageous conduct in any board of guardians? I believe this will never be remedied until we have a fair scale of remuneration, with a medical commissioner to insist on its application. I remain, Sir, your most obedient servant,

CHIRURGUS.

Colchester, May 31, 1842.

BRENTFORD UNION.

To the Editor of THE LANCET.

SIR,—I have been requested to forward to you the following report of our proceedings, and to request you will be kind enough to allow it to appear in the ensuing number of THE LANCET. I take the opportunity to thank you for your polite attention on former occasions, and have the honour to be, Sir, yours very obediently,

HORATIO G. DAY, Hon. Sec.

Isleworth, June 1, 1842.

The medical practitioners resident in the several districts comprised in the Brentford Union, having received notice from the board of guardians of the amount of remuneration proposed for the medical officers, held a meeting at Brentford, on Tuesday last, May 31, when the following resolution was unanimously agreed to be presented to the board at their next meeting:—

"We, the undersigned medical practitioners, having duly considered the recent regulations of the poor-law commissioners, are of opinion that the rate of payment proposed by the board of guardians for the different districts in this union, is inadequate

to insure proper attention and the best medicines.

(Signed)

"*Acton.*—William Clubbe, Henry Day, John Salt.

"*Old and New Brentford.*—F. A. B. Bonney, John Farrell, H. J. Radcliffe, Henry Richards.

"*Chiswick and Turnham-Green.*—W. W. Cox, F. C. Dodsworth, C. C. Eyre, W. Lambton, H. J. Leigh, R. W. Loadman.

"*Ealing.*—Henry Wilkins.

"*Hanwell.*—Thomas Haffenden, J. C. T. Nicoll.

"*Hounslow.*—G. J. S. Camden, Joseph Chapman, C. B. Emmott, R. A. Frogley, John Henley, Wm. Wheadon.

"*Isleworth.*—W. M. Case, H. G. Day, J. Mackinlay.

"*Twickenham.*—John S. Alger, C. C. Clark, J. Simoëns."

The above list includes all the practitioners in the Brentford Union (with two or three exceptions) who take any interest in parochial attendance; three or four of the senior professional men declining to affix their names to any memorial connected with the subject. It may, in fact, be considered to have met with the unanimous concurrence of the medical men in the union, and therefore cannot fail to have its proper effect upon the guardians.

ROYAL MEDICAL AND CHIRURGICAL SOCIETY.

Tuesday, May 24, 1842.

Dr. Williams, President.

Cases of Malformation of the Heart. By Bell Fletcher, M.D., Physician to the General Dispensary, Birmingham.

The first of these was one of aneurism and dilatation of the pulmonary artery, malformation of the heart and arteries. The patient was a young woman, aged 19, who, with the exception of slight cough, had enjoyed good health up to the age of sixteen; since that time she has had frequent attacks of pain in the chest, cough, and dyspnœa. She was first seen by the author in August, 1839, when she had cough and dyspnœa, with slight hæmoptysis. On the left side of the sternum, between the second and third ribs, there was a superficial pulsation, with purring tremor, and a loud rasping souffle; the impulse of the heart was normal. She recovered under the use of the remedies employed, but had several similar attacks between that time and December, 1840, when she had severe inflammation of the lungs. At this time the pulsation of the chest mentioned above was more extensive, the souffle louder, and it had assumed a *see-saw* character, masking the sounds of the heart. The patient lingered for some time, and though relieved by treatment, died rather suddenly on May 1.

Examination after Death.

Our account of the appearances must be confined to the state of the heart and great vessels. The heart was about twice its natural size; its parietes of the natural thickness; the dilatation being equal in all the cavities. The right auricle, excepting the dilatation and hypertrophy, perfectly normal; the tricuspid valves healthy. The auriculo-ventricular opening is dilated, so as to render the valves somewhat ineffective. In the right ventricle the columnæ carneæ are thicker than normal; the orifice of communication with the pulmonary artery is much dilated. Immediately below the opening is a communication with the left ventricle, which measures about one-third of an inch across, and one-eighth from above downwards. The two anterior semilunar valves are perfect, of the normal size, but much thickened; the posterior one imperfect, being so narrow and constricted in its middle portion as to be a mere band-like projection from the artery. The pulmonary artery is much dilated, being five inches and three-quarters in its greatest circumference (internal measure); the branches of the pulmonary artery are much dilated. The left auricle is healthy, save the dilatation and hypertrophy; the auriculo-ventricular opening is dilated; the mitral valve is normal in structure, but not sufficient to close the dilated opening perfectly. In the left ventricle the columnæ carneæ are thicker than usual, and at the base of the cavity is seen the opening of communication with the right ventricle. The opening of the aorta is rather dilated; semilunar valves healthy. The ascending aorta is dilated about one-half beyond its normal size. The distribution of the arteries from its arch may be considered more as divisions of the artery than branches. After giving off these it is contracted to about one-third of its capacity in the ascending portion, and the walls of the vessel are thinned in this situation. This contraction extends from the root of the left subclavian to the joining with the ductus arteriosus, where the artery is still further contracted by a hardened thickening of the coats, projecting into its interior, so as to contract the capacity again to about one-third: so that at this point the capacity of the artery is not more than one-ninth of its ascending portion. Just on the distal side of the constriction is the communication with the ductus arteriosus, and immediately below it, the artery is dilated to the usual size of the descending aorta.

The second case was one of cyanosis; the subject of it a young man, aged twenty-one. He had great dyspnœa, palpitations, anasarca, and a blue state of the skin generally. The heat of the body in the axilla, or mouth,

was not more than 80° of Fahrenheit; the state of puberty was not developed. The impulse of the heart was greater than normal; there was a bruit de soufflet beginning with the first sound of the heart, sufficiently long to mask the second; it was heard most distinctly under the middle bone of the sternum.

Examination after Death.

The heart and large veins enormously distended with dark-coloured blood; the organ much enlarged and hypertrophied. The septum of the auricles so totally absent as to convert the auricles into one; the veins of the general circulation enter it on the right side of a line drawn in the situation of where the septum should be. The auriculo-ventricular openings are large, and between them the septum of the ventricles is much notched out. The orifice of the aorta is contracted to a third of its usual capacity, and its semilunar valves are diseased and ineffective. The liver much congested and enlarged, about a third more than its usual size.

Dr. WILLIAMS remarked, that the first case presented a singular, and he believed unique, example, of aneurism of the pulmonary artery. This case also showed the correctness of the axiom laid down by Laennec, that when there was ossification on the right side of the heart, there was usually a communication between the two sides of the organ. The second case was an interesting illustration of the two auricles communicating and forming one cavity.

Dr. GREGORY inquired whether any member had seen a cutaneous affection in a child affected with cyanosis. He had himself never seen a child with this affection who had any disease of the skin. He had been called, however, some short time since, to vaccinate a child with cyanosis; the vesicle was in every respect normal, except that the areola, instead of being crimson, was purple.

Dr. FLETCHER, in answer to a question, stated that both ventricles of the heart in the second case were proportionally dilated and hypertrophied.

Dr. ADDISON said, there were some important points connected with the first case. He had some time ago in the society attempted to show that when there was a strong, superficial bruit, of a very loud character, with the first stroke of the heart, and obscuring the other sound, that it was usually dependent on a communication between the two ventricles. What was the cause of the appearance of the pulmonary artery in the first case? He questioned whether the reason of Laennec on this point was correct. He (Dr. A.) believed the dilatation to be dependent on some obstruction in the left ventricle, by which additional work was given to the right ventricle: the violence consequent upon this produced the dilatation. He related the case of a young woman who had suffered from disease of the heart for some time, the condition of the mitral valve producing an obstruction. The right ventricle became hypertrophied, the right auricle dilated, and this dilatation extended to the pulmonary artery throughout its minute ramifications in the lung. The tunics of the artery presented atheromatous deposits. The reason why disorganisation of the right side of the heart was less common than that of the left, depended on the circumstance of the former being less liable to violent action than the latter. Dr. Addison then entered into an explanation of the causes of the abnormal sounds in the cases related.

Dr. CURSHAM referred to a statement made in the paper regarding blood-letting in cyanosis. He might state, for the information of the author, that he had seen a case of this disease in a boy who was admitted a short time since into the Marylebone Infirmary, and in whom blood-letting to a considerable extent was resorted to. The boy, on admission, suffered so much from dyspnœa, that his life was despaired of; as the blood flowed he became relieved. He died afterwards: there was no post-mortem. There was a loud, superficial bruit over the heart in this case.

A paper was afterwards read from the pen of Dr. Hocken, but the "abstract" furnished to us merely gives the title of the production, and expresses an opinion as to its merits.

The society adjourned to June 14.

MEDICAL SOCIETY OF LONDON.

Monday, May 30, 1842.

Mr. PILCHER, President.

DISCUSSION ON INSANITY.—THE NON-RESTRAINT SYSTEM.—CLOSE OF THE SESSION.

THE discussion on mania was resumed.

Mr. HIRD remarked, that acute mania had been considered by some of the speakers as identical with phrenitis. There was, however, generally speaking, a wide difference between the two diseases; for although the symptoms, so far as the mind was concerned, might be similar, the bodily symptoms were altogether different. Sometimes, indeed, these latter symptoms might indicate the necessity of blood-letting; but as a general rule this was not necessary. In some cases of mania a peculiar condition of the nervous system indicated the employment of narcotics; while in others, tonics would be administered with most advantage. Blisters would be serviceable when applied near the head in those cases in which blood-letting had been premised. He had tried elaterium, but not with much advantage.

Dr. LEONARD STEWART said, that we must be guided in our treatment of mania by the physical symptoms. He related a case to show that in mania, attended with much physical debility, tonics and even stimulants were necessary; but that after the lapse of a few months, when the health and strength of the patient became restored, small and repeated blood-lettings were resorted to with great advantage.

Mr. PROCTOR cautioned young practitioners against measuring the necessity of blood-letting by the degree of mental excitement present, for irritation of brain might exist with a state opposed to that of inflammation.

Mr. CLIFTON remarked, that mania might be accidentally complicated with inflammation of the brain, but that inflammation was by no means necessarily present. He inquired the experience of members regarding the moral treatment of insanity, and instanced the astonishing results of Dr. Conolly's non-restraint practice at Hanwell. The humane system had gained attention among the public, the profession would do well to inquire fully into the subject.

Mr. HEADLAND said, that he agreed with Cullen in placing mania under the class *neuroses*, order *adynamia*, and that consequently he did not think it an inflammatory disorder. If inflammation did exist, it was only as a consequence of the disordered condition of the mind. He thought it quite inconsistent with our observations on insanity to consider it as Mr. Solly did, always associated with inflammation of the brain; for were this the case, anti-inflammatory remedies would always be of service: whereas, we knew, on the contrary, that they were often followed by an aggravation of the symptoms. The more rational treatment of insanity was that which brought into useful action those faculties of the mind which remained healthy. To arrive at a proper mode of treatment in each case, we must be acquainted with the previous history of the patient. Whatever, however, was the cause of mania, sleep, if it could be procured, was always of advantage, and would do much towards the restoration of the health of the patient. The sooner this sleep could be procured the better. The preparations of opium for this purpose, might be exhibited at any stage of the disorder, where the tongue was not dry or loaded; and where it gave evidence of anything like tremor, or where there was tremor of the body generally, and moisture of the skin. Under these circumstances, whatever the state of the pulse, opium might be safely and advantageously employed.

Mr. DENDY believed that the original cause of mania was a metaphysical one, but that in all cases there was an abnormal condition of the brain present, but which was not necessarily inflammatory.

In regard to the non-restraint system, Mr. DIAMOND and Mr. ELLIOTT contended that there were some cases of mania in which restraint could not safely be dispensed with; such were cases in which patients eat their own fæces, or had a predisposition to suicide.

Mr. CLARKE remarked, that a fortnight ago, in Bethlem, where, some time since, it was said that restraint could not be dispensed with, there was not one patient out of three hundred and sixty under restraint! The last reports of the Gloucestershire, the Edinburgh, and other asylums, showed that the system of Dr. Conolly was as practicable as it was humane.

Mr. CLIFTON said, if the treatment pursued by Dr. Conolly in the Hanwell Asylum was found to answer, would the better classes of society submit, in private asylums, to that restraint which was dispensed with in the cases of their poorer brethren in public institutions?

Mr. PILCHER had been interested with the discussion which had occupied the society during the last three evenings. The difference of opinion was more imaginary than real. He proceeded to say that he considered mania was essentially a physical disease, and that it was situated in the grey matter of the brain. He referred to phrenology, and thought that it threw more light on insanity than any of the other systems which had been promulgated.

This being the last meeting of the session, the society adjourned until September 26th.

PHRENOLOGY AND PHRENOLOGISTS.

To the Editor of THE LANCET.

SIR,—The letters of your correspondents "Spectator" and "W. H. Brooks" in your Number of Saturday last provoke a few remarks, for which I have to crave the indulgence of a place in your columns. I must beg to remind those gentlemen, and your readers generally, that up to this time the facts brought forward in Mr. Sampson's letters remain uncontradicted, and the arguments unrefuted; and I cannot help thinking that Mr. Sampson's opponents would do greater service to science and to themselves if they would investigate the *truth* of his conclusions, instead of indulging in vain speculations as to the consequences which those conclusions may involve. Whether the views in question be "most dangerous" as in a former letter your correspondent "Spectator" affirms, or whether they favour "the untenable positions of fatalism" as Mr. Brooks seems to imagine, are questions beside the real one. *Are they true or false?* If they are true, it is idle to talk of their being "dangerous;" *truth* can only be dangerous to *error*. If they are false, let it be shown where the fallacy lies—let it be

pointed out fairly, and in precise unequivocal terms. The proposition, consciously or unconsciously affirmed by "Spectator" in his last letter—the proposition which he helps to establish by his quotation from Hazlitt, viz., that beings are to be considered "moral and accountable" in the ratio of their susceptibility to "moral motives," is one to the truth of which Mr. Sampson will, no doubt, cheerfully assent. The question still remains open, viz., Are all men equally susceptible to the influence of moral motives? I pass by your correspondent's statement regarding "the proper province of phrenology," being utterly at a loss to reconcile it with even a moderate knowledge on his part of the elementary principles of the science in which he professes to put faith. Phrenology, as explained by Gall, Spurzheim, Combe, and others, teaches us, 1st, that the brain is the organ of the mind; 2nd, that the brain is a congeries of organs; 3rd, that, *cæteris paribus*, size is the measure of power; 4th, that susceptibility and activity depend upon the temperament, the influence of which extends alike to all the cerebral organs. Phrenology teaches us, that men with heads like Melancthon, experience lively emotions of a moral character, and that such men feel strongly impelled to do that which is *right*. That men with heads like Hare or Burke may recognise *intellectually* the difference between right and wrong; that is, they may perceive that a particular action is of a class *called wrong* by the world, that another is of a class *called right* by the world; but that the *sensible appreciation of the morality of the act*, the feeling or sentiment which can alone furnish a *restraining motive*, is in them extremely weak, and liable to be overborne by strong temptation. The question evidently suggested to Mr. Sampson's mind by this doctrine, is not—What is then the degree of such a man's moral guilt in the sight of his Maker? Such a question would be at once impious and absurd. None but the "Searcher of all hearts" himself can determine what is due from each of his creatures to Him; but rather this, To what extent are we on earth justified in *taking vengeance* upon our fellow-man for "crimes" committed by him? Mr. Sampson may be right, or he may be wrong: one thing is clear, *he is in possession of the field of action;* and so he is likely to remain, until "a stronger than he" shall overcome him with the weapons of fact and argument which he employs so ably. I am, Sir, your faithful servant,

R. W. HEURTLEY.

Kensington, May 30, 1842.

NOTE FROM DR. DICK.

To the Editor of THE LANCET.

SIR,—I must make a thousand apologies to you and your readers, for returning once more to that question of phrenology, *vexata* (I may surely *well* add) *usque ad nauseam;* but justice to myself requires me to state, explicitly and once for all, that (contrary to what "Spectator" supposes) I *am* a phrenologist! I believe, and in my late papers have repeatedly asserted, the brain to be the organ of the mind; which opinion or admission *regularly constitutes me a phrenologist*. It is true, indeed, that my phrenological creed (sceptic as I must appear to the orthodox Messrs. Sampson and Combe!) consists of but a *single*, and that, too, a most general doctrine; yet for going beyond which I deliberately maintain we have NO data. That single doctrine, which I *purposely* clothe in the most general terms possible, is, that *there seems to exist a relation between the anterior part of the brain and the intellect*. What *I* consider the amazing credulity of Messrs. Combe, Sampson, &c., a credulity which partly surprises, partly amuses me, will by no means prevent me from avowing myself a phrenologist in *my* way, which it will be perceived is widely different from, though as real as theirs. I may add my firm and solemn conviction, that until it be generally understood, as it long since *ought* to have been, that sound and rational phrenology and Mr. Combe's theories, are things just as opposite as light and night, an eternal hostility will exist to the former, in many or in most intelligent minds.

Another grand tenet of mine, of which I hope and believe most of your readers are convinced, is, that so long as a man is *intellectually* sane he is *morally* responsible. The utopian (or not to misapply a word of pleasing import) the wildly chimerical character of the contrary view, will ever be a safe antidote to the dangerousness of it. I am, Sir, your obedient servant,

ROBERT DICK, M.D.

London, May, 1842.

MEDICAL ASSOCIATIONS FOR THE PREVENTION OF ILLEGAL PRACTICE.

To the Editor of THE LANCET.

SIR,—I beg leave, through the medium of your widely-circulated Journal, to call upon the legally-authorised practitioners of medicine in this metropolis to form district associations for the suppression of illegal practice. It is notorious that such is carried on to an enormous extent; but under the influence of the feeling that what is the business of everybody is the business of nobody, nothing is done: some cases, few and far between, are exposed, but the great mass of the evil is allowed to exist undisturbed. It is certainly by no means a pleasant thing for an individual to appear as an informer, but the odium would not be thus felt if the matter

were to be taken up by an organised body. The Legislature has given us power. *Habemus enim hujus modi senatus consultum; verùm tamen inclusum in tabulis tanquam gladium in vagina reconditum.* Let the Act no longer be a dead letter, but bring the delinquents to the light that their foul deeds may be manifest.

The druggists have thrown down the gauntlet, and are determined to press for right to practise, though for the present modestly disclaiming any intention of being visiting practitioners. Oh! the sly rogues! *Principiis obsta.* When they are allowed to *do for* her Majesty's subjects in the shop or back parlour, they will then try and *do for them* abroad. Mark their cunningly-devised manifesto:—"A chemist and druggist is a person who has been regularly apprenticed to a vendor of drugs or dispenser of medicines, and who does not *profess* to act as a visiting apothecary or surgeon; it is impossible to define accurately the boundary between the performance of this duty and giving advice in simple cases." "If practising medicine means recommending or administering a remedy for any ailment or disorder of the body or mind, the druggist would be liable to fine, &c., every hour of the day." The foregoing quotations are from p. 177 of the "Pharmaceutical Journal." After such frank avowals as the above, there can be no mistake as to the present position and future intentions of the druggists. They have fairly come out.

The instances of injury that these ignorant prescribers inflict upon the public are numerous and frequent. The writer has had a patient four days in bed, and now confined to the house, in consequence of one of these men prescribing a violent corrosive external application to be applied to the arms and thighs. The poor woman will not give up the name at present, but I hope to find it out, and to make an example of the delinquent.

There are many who profess to act as visiting apothecaries without any legal qualification, and these are well known. There are few parts of the lower districts of town which are not infested by these fellows: so that it becomes a moral duty of the qualified practitioners to rid the town of such nuisances. I am, Sir, your most obedient servant,

AN APOTHECARY.

London, April 25, 1842.

PUBLIC CONVENIENCES.

To the Editor of THE LANCET.

SIR,—Having seen in the newspapers of three or four days since an account of a man's dying in consequence of the privilege of a necessary being denied to him, I am induced to write to you on a subject which has doubtless occurred to the minds of most persons, but which, from some unaccountable feelings of false delicacy, has never been made a subject of public inquiry. I allude to the almost total want of public necessaries, and of skreened places for making water in, in the centres of the great towns of England. In Paris and other continental towns skreens or retired places are most numerous; as, for instance, in the Boulevard des Italiens, where they occur every sixty paces, and the constant requisition to which they are put by all orders testify well to their use; whereas in London a man may walk literally for miles without meeting with a single place to which he can possibly retire. If he walk down any alley or side street which seems favourable for his purpose, the notice of "no nuisance" is sure to meet his eyes, the sanctity of which command is enforced by law. Now, Sir, I appeal to your medical experience whether a much more common disease exists, at least among men of advanced years, than a degree of prostatic irritation, and whether a much greater degree of inconvenience can be imagined than the inability of complying with nature's calls under those circumstances. It is needless to enumerate the various diseases, as gravel, stone, stricture, &c., which act in a similar way on the bladder, and all of which are common.

I, therefore, am anxious to draw the attention of the public, and more especially of those whose authority in such cases would be first appealed to—the medical public, to the necessity which exists for the construction of public necessaries and skreens in the centres of all large towns, not placed in conspicuous situations, but down small streets and in other convenient places. If an individual were to erect water-closets for the use of gentlemen and ladies, it no doubt would prove a paying speculation; but there is an absolute duty that necessaries and skreens should be placed by those whose premises the law protects from indiscriminate nuisance. Earnestly submitting this to the notice of yourself and of your readers, I remain your obedient servant,

G. A.

May, 1842.

ROYAL COLLEGE OF SURGEONS IN LONDON.

LIST of gentlemen admitted members on Friday, May 27, 1842:—Thomas Sutfield Rising; James Walsh; Ambrose Willy; William Barclay Browne Scriven; William Samuel Jones; Richard Walsh; John Kilroy; Frederick R. Spackman; Edward M'Donnell; John Drummond; John Vondy. Admitted Monday, May 30, Henry Sutton Lyford; Stephen Orson Lane; Thomas Ber-

nard Ryan; Thomas Esmond White; Thomas Upton Nutt; John Simpson Rutter; George Senior; Edmonds Robert Richardson; William Marwood Kelly; John Broadhurst; Richard Tomkinson.

BOOKS RECEIVED.

Domestic Homœopathy; or Rules for the Domestic Treatment of the Maladies of Infants, Children, and Adults, and the Conduct and the Treatment during Pregnancy, Confinement, and Suckling. By John Epps, M.D., Director of the Royal Jennerian and London Vaccine Institution. London: Sherwood and Co. 1842. Pp. 200.

A Catalogue of Works in all Departments of English Literature, Classified: with a General Alphabetical Index. In which the full Titles, Sizes, Prices, and Dates of the last Editions are given. London: Longman and Co. 1842.

The Anatomist's Vade-mecum: a System of Human Anatomy. By Erasmus Wilson. With 167 Illustrations, by Bagg. Second Edition. London: Churchill. 1842. 12mo. Pp. 600.

TO CORRESPONDENTS.

PRIVATE LUNATIC ASYLUMS.—(*From a Correspondent.*)—When the subject of private asylums is before the House of Commons, it is much to be wished that the inspecting commissioners should have conferred on them the power not only to examine as to who is sane or insane, but also to compel the proprietors of the country asylums to pay more attention to the comfort and improvement of their patients. They will find in what are considered to be "most respectable" establishments, that the houses are not properly ventilated; that the walls are thin, the divisions seldom whitewashed; the upper sashes of windows not made to open, no shutters, no blinds in some rooms; and that patients in the most feverish state are placed in apartments under the roof somewhat resembling ovens, which, of course, brings on excitement and noise, and then every aperture is closed to prevent sound from escaping. All these evils come not under the notice of visiting magistrates, nor are they at all aware of the many accidents that occur to patients, in the form of attempts to commit suicide and broken limbs, which are kept profoundly secret, lest it should prove requisite for the proprietor to reside at the house; all such accidents occurring from the carelessness of servants, in the absence of proprietors; the too often ignorant, half-paid house-surgeon being absent also, and the servants left to their own discretion. Surely the house-surgeons, also, should be duly qualified persons! And could any asylum be pointed out in which any effort is made for the amusement or recovery of the upper classes? Alas! no: the proprietors are frequently occupied by their own pleasures, and the patient is often regarded as only worthy to be kept in existence so far as he contributes to their gratification and luxury, money procuring only his continued confinement; the situation of the affluent patient becoming more hopeless than that of the pauper. Little benefit will arise if there be not more control exercisable over the proprietors than now exists.

No advantage commensurate with its bulk would be obtained by the publication of the examination at St. Andrew's.

Mr. Reed.—The clavicle and scapula should unquestionably be regarded as bones of the arm; and should be allowed for according to the scale of remuneration published at p. 17 of the present volume of THE LANCET.

THE MONEY-TEST OF EDUCATION.—The following ignorant suggestion has recently been addressed to the governors of the Royal Veterinary College in London, by certain memorialists, who imagine the possession and payment of money to afford evidence of education and ability. They intimate that "by increasing the present fee (payable by students at the college) to thirty guineas *or more*, according to the number of lecturers, your memorialists are of opinion that your professors would be better remunerated; and that it would form one check against the admission of illiterate students. They would further remind you, that in no one of the public schools of medicine, to which an hospital is attached, are the scale of fees one-fourth part so low as at the Veterinary College."

The *Cork Constitution* should have been acknowledged as received, instead of *not* received, in our Number of May 21.

THE NEW UNION MEDICAL ORDER.—When does the new medical order issued by the poor-law commissioners come into effect? I received a note, a few days back, from the clerk of the Bromsgrove board of guardians, stating that it would not come into operation in this union until after the 25th of March, 1843. By informing me whether the board have the power of withholding the benefit of it from the different medical officers of the union until that period, you will oblige your obedient servant, J. S. S.—*** We reply to our correspondent, that such is the power of their high mightinesses at Somerset House, that they can, without hindrance or existing remedy at law for the delay, postpone the operation of the order for any number of months or years that they may choose. The time is wholly at their option, and it is beyond our ability to prophesy the *when* respecting the acts of men possessed of such irresponsible powers.